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In the final issue of each volume, I am pleased to acknowledge the reviewers who contribute so much both to our peer review process in general and to the papers that are ultimately accepted for publication in particular. In the 1996–97 academic year, 919 scholars assisted the profession in this way, many of them multiple times, providing reviews for more than 500 manuscripts submitted to the *Review.* Their names will be found following the book reviews. The index for Volume 91 is also provided at the end of this issue.

Looking to the future, Volume 94 of the *Review* will be published in the year 2000, only eight issues away. While the *Review* does not commission articles, and it is doubtful that we would devote any issues to single themes, I hope scholars who are working on significant political science research that has some connection to the millennium will think of submitting such work here over the next few years, so that it may be published in 2000 or 2001 (according to your millennial beliefs). The turn of the century is a natural time for stock-taking, so critical evaluations and assessments of research traditions and literatures as well as reports of original research projects will be welcome. As usual, all submitted articles will go through our normal peer review process.

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The *American Political Science Review* strives to publish scholarly research of exceptional merit, demonstrating the highest standards of excellence in conceptualization, exposition, methodology, and craftsmanship. Because the *Review* reaches a diverse audience of scholars and practitioners, contributors should demonstrate how their analysis illuminates a significant research problem, or answers an important research question, of general interest in political science. For the same reason, authors should strive for a presentation that will be understandable to as many scholars as possible, consistent with the nature of their material.

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The Gains Debate: Framing State Choice

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The theoretical bifurcation of international relations theory into (neo)realist and (neo)liberal camps has resulted in a "gains debate" that says little about an empirical world in which states exhibit both relative and absolute gains pursuit. This article deploys prospect theory in an attempt to move beyond the gains debate. The intent here is synthetic. By bringing the predictions of both perspectives under a single theoretical umbrella, we can model a broader set of state behavior. The thesis developed demonstrates that states in a gains frame pursue absolute gains and are risk averse, while states in a losses frame pursue relative gains and are risk acceptant. This hypothesis is assessed against the behavior of the European Community in the formation of the Montreal Protocol, a regime intended to protect the earth's protective ozone shield. The new model accurately predicts the timing and content of shifts in EC preferences, suggesting that a synthesis of realist and liberal approaches is possible.

State attempts to secure international cooperation are confined by the distribution of power across relevant actors, the constraints imposed by the international system, and the intentions and actions of other states. Recently, the theoretical discussion surrounding state intentions has left us with two distinct and competing models of state choice. Liberalism asserts that "the existence of international regimes composed of sovereign entities who voluntarily eschew independent decision making" evidences a world in which states maximize absolute gains (Stein 1983, 134). Realism counters that "the fundamental goal of states in any relationship is to prevent others from achieving advances in their relative capabilities" (Grieco 1988, 498). The resulting debate has "dominated much of international relations theory for the last decade," to the extent that it is now often "commonplace for an article about some aspect of international theory to begin by locating itself in terms of this debate" (Powell 1994, 313). This polarization into competing camps has also meant that "these two approaches and the debate between them have failed to contribute as much as they might have to international relations theory" (Powell 1994, 313).[1] Structured discussion between adherents of both sides has not resolved the matter. Indeed, if anything, it has become acrimonious.[2]

Current thinking is unlikely to resolve the gains debate. The problem derives not so much from our theories about international politics as from the model of decision making that often underpins them. While realism and liberalism offer contrasting goals for states, they both employ classic microeconomic rationality to model state choices. Under this representation, preferences are exogenous, unexplained, and therefore often given by assumption. To construct *general* theories of state choice, realists and liberals must first independently settle state gains preference: Preference must be established prior to choice. The origins of the disagreement, then, lie in the conjoining of the microeconomic assumption with a necessarily fixed and exogenous source of state preference. Both realism and liberalism deduce preference from characteristics argued to be inherent and permanent features of anarchic systems.[3] Yet, there is little agreement about the imperatives of anarchy, and contested interpretations support both realist and liberal assumptions.[4] In addition, it is likely that the relevant characteristics of anarchy are socially constituted (Wendt 1991). As the exogenous source for a *fixed* state preference, anarchy is at best problematic and therefore a questionable foundation upon which to construct a universal theory of state goals.

Students of international relations may benefit from exploring alternatives to classic rationality in the construction of a general model of state choice that treats preferences both as endogenous and connected to context. For example, prospect theory comes from an empirical investigation into the structure of choice and reveals that individuals treat gains and losses differently. Within this tradition lies the possibility of constructing a general model of state choice which can simultaneously accommodate both relative and absolute gains. Instead of assuming a fixed state preference, the focus of this paper is upon the state's assessment of changes in the status quo. Proposed here is a two-step model in which the state first evaluates the effect of change upon its own capacities and then, under this

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[1] For ease of reading I use interchangeably the terms realism and neorealism as well as liberalism, neoliberalism, and neoliberal institutionalism. While the ideas embodied in these literatures will be discussed below, realist writings include Gilpin 1975, 1981, 1987; Grieco 1988, 1990; Krasner 1991; Waltz 1979. Liberal writings include Axelrod 1984, Axelrod and Keohane 1985, Stein 1983. Other studies addressing various aspects of the debate itself include Mastaduno 1991; Snidal 1991a, b; Suzuki 1994.

[2] See Grieco 1993, Powell 1993, and Snidal 1993. A structured discussion between adherents of both perspectives is found in Baldwin 1993.

[3] Briefly, in terms set out by Grieco (1988), realists maintain that the lack of hierarchical order under international anarchy constitutes a permanent survival threat. Liberals downplay the survival imperative and argue instead that the various incentive structures under anarchy (e.g., prisoner's dilemma) present opportunities for states to maximize absolute gains, and such pursuit is not necessarily subordinate to a concern about relative position.

[4] Even foundational concepts like the lack of formal order under anarchy are in dispute. See Milner 1991.

new assessment, makes comparisons with potential rivals. When the status quo is viewed to be detrimental, states pursue relative gains. When the status quo is viewed to be beneficial, states pursue absolute gains. The concern for relative position derives from the threat of leverage, which is most acutely felt whenever the state experiences loss. Absent the threat of leverage, states feel free to pursue absolute gains without concern for the capacities or intentions of other states.

This new framework is assessed against the behavior of the European Community (EC) during the Montreal Protocol negotiations to limit the release of ozone-destroying chlorofluorocarbons (CFCs) into the atmosphere. Prior to negotiations the EC experienced both relative and absolute gains. Realism predicts that EC policy will attempt to protect recent relative gains in the international CFC trade and solidify its newfound position as the dominant international supplier to the global market. Liberalism, by contrast, predicts protection of recently increased production levels. These are not necessarily exclusive goals, but within the context of the Montreal talks the EC was never able to pursue both. Because both realism and liberalism view state goals as fixed, we would expect a consistent policy as the EC is forced to choose between relative and absolute gains. Yet, EC policy was not consistent; it shifted from protection of absolute production levels to the pursuit of relative gains. On this point alone the traditional division between realist and liberal theory is challenged. By contrast, the behavior of the EC is consistent with the new framework offered here, prospect theory, which predicts changes in gains policy by context. The discussion below demonstrates that under a gains frame the EC pursued absolute gains. When the social frame was changed to losses, the EC abandoned absolute gains and sought relative position.

Observers and participants to the negotiations have hailed the Montreal Protocol not only as a general model for achieving international cooperation but also as a concrete framework with which to address emerging global environmental commons dilemmas (Benedick 1991, Morrisette 1991, Parson 1992). The process typically has been characterized as states concerned with ecological stability carefully crafting the contours of an agreement in such a way as to get reluctant states to acquiesce. While the details of the Montreal agreement are important, such a characterization misses the most salient fact about the politics surrounding ozone depletion. The central concerns of the EC were economic, and not until those were addressed did an agreement become possible.[5] A detailed examination of the diplomatic history of the Montreal talks confirms this. The fact that an issue with such clear global ecological consequences can become mired in a disagreement over the costs of regulation speaks to the salience of the issues raised by the gains debate.

The sections below first identify those aspects of prospect theory from which a theory of state choice will be derived. Next, prospect theory is integrated into a theory of leverage to produce a framework that predicts when states will pursue relative or absolute gains. Finally, the new framework is evaluated against the behavior of the EC during the Montreal Protocol negotiations. A brief summary and conclusion are then presented.

THE FRAMING HEURISTIC

While recent studies have successfully applied prospect theory to empirical issues in international relations (Farnham 1994, Stein and Pauly 1993), such work draws only upon the observed propensities for risk acceptance and aversion that define the framing effect.[6] This effect is itself deduced from a set of core observations about the form of a decision maker's value function. This article explores the possibilities for theory construction that follow from these same core observations by deducing a set of propositions in much the same way the framing effect was derived. Not only does prospect theory provide an even more fertile ground for theory construction than already has been established, but also theoretical and empirical progress is possible by deploying alternatives to microeconomic rationality.

As an alternative to classic rationality, prospect theory comes from the empirical investigation into the structure of individual choice. The cornerstone of prospect theory is the identification of an S-shaped value function, which is constructed from two commonly and empirically verified observations. First, there is a diminishing value from continual increases in gains. The subjective difference between "a gain of $100 and one of $200 is more significant than the difference between a gain of $1,100 and one of $1,200" (Kahneman and Tversky 1982, 162). This is also true for losses, so that the difference between a loss of $100 and a loss of $200 is more significant than the difference between a loss $1,100 and a loss of $1,200. Second, losses subjectively hurt more than gains feel good, so that the sting of loss is more acute than is the enjoyment derived from an equal gain (Quattrone and Tversky 1988, 721). Combined, these observations suggest the value function shown in Figure 1—concave in the domain of gains and convex, but steeper, in the domain of losses.

The most familiar implication of this S-shaped value function is the framing effect, such that individuals are risk averse in the domain of gains and risk acceptant in

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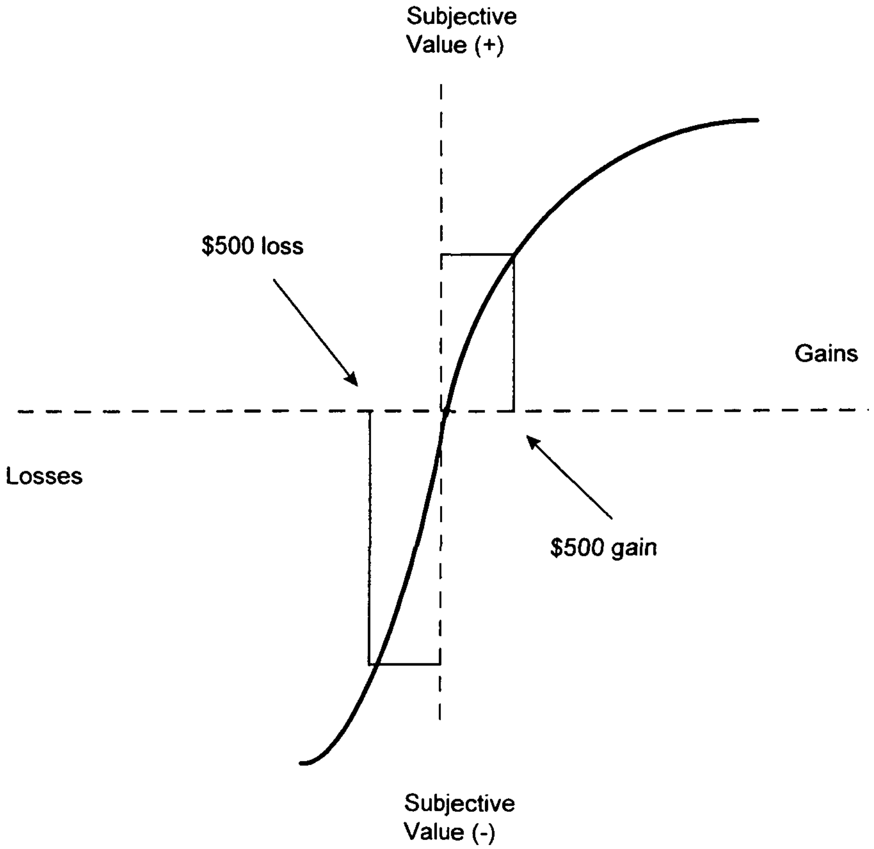
[5] Much of the literature on the Montreal Protocol either ignores or downplays the economic disagreement between the United States and the EC, and none of it makes the claim that this was an economic rather than ecological issue. See, for example, Cook 1990, Haas 1992, Litfin 1994, Parson 1992, Soroos 1991. Those who look to the protocol as a blueprint for conducting talks on issues like global warming are therefore instructed by a discussion which explicitly addresses the gains concerns of states. Any agreement on global warming, which promises to be much more complicated, will have to address the economic issues resolved in the Montreal Protocol.

[6] See also the special edition of *Political Psychology* 13(2) 1992, dedicated to applying prospect theory to international relations.

FIGURE 1. The Gain-Loss Value Function



the domain of losses.[7] The essential characteristics of framing are best illustrated through example.

> *Gains Frame*: Imagine a choice between two options that imply personal gains. Option 1: A sure gain of $80.00. Option 2: A risky venture with an 85% chance of winning $100 and a 15% chance of winning nothing.
> *Losses Frame*: Now imagine a second set of options that imply personal losses. Option 1: A sure loss of $80.00. Option 2: An 85% chance of losing $100 and a 15% chance of losing nothing.

When confronted with the first choice set, most people prefer the sure gain over the risky venture, even though the expected value of monetary gain is $5.00 less. In the second set, individuals prefer the gamble over the sure thing, even though the expected value of monetary loss is $5.00 greater. As Quatrone and Tversky (1988, 722) put it: "According to prospect theory, an individual's attitude toward risk depends upon whether the outcomes are perceived as gains or losses, relative to the reference point." While individuals are risk avoiders in the gains quadrant, the data show them to be risk seekers in the losses quadrant. As a result, "the same decision can be framed in several different ways; different frames can lead to different decisions," so that "framing effects arise when the same objective alternatives are evaluated in relation to different points of reference" (Kahneman and Tversky 1982, 164, 165). For my purposes, the important point here is that the decisional determinant is not the expected outcome but how the choice is perceived relative to the reference point, and the explanation for individual choice is located in perceptions of gain or loss relative to the status quo.

CHOOSING THE STATUS QUO

If prospect theory is to contribute to a theory of state action, then a method is needed for determining when the options facing the state are framed as a choice between gains or between losses. Experimentally, this is easily done by giving respondents carefully structured choice sets that can be unambiguously classified as gains or losses (Quattrone and Tversky 1988). Whenever the choice set facing states looks something like the lottery presented above, then prospect theory can be straightforwardly applied to state choice. It is often the case, however, that states face choices not between two such narrowly defined alternatives to the status quo but between the status quo and some change. The decision confronting the state is between accepting the change or attempting to reverse it; or, if change is foreseen, then between a passive acceptance

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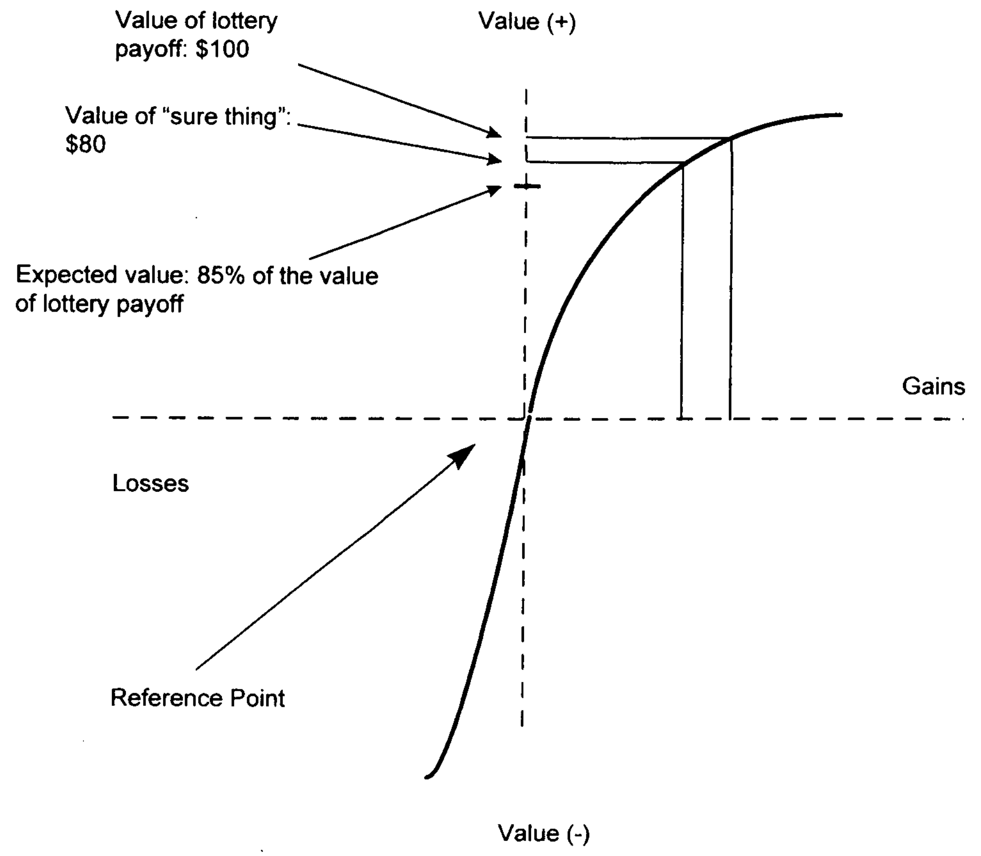
[7] For a detailed explanation of why an S-shaped utility function leads to the framing effect, see Kahneman and Tversky 1982.

FIGURE 2. A Lottery Choice Example


Value of lottery payoff: $100
Value (+)
Value of "sure thing": $80
Expected value: 85% of the value of lottery payoff
Gains
Losses
Reference Point
Value (-)

and a proactive forestalling. In these instances prospect theory may well be inappropriate, because selecting the status quo is not an option in the standard choice set.

Yet, changes in current conditions are initially evaluated against prior circumstances, and there is good evidence to suggest that decision makers treat the past as a reference point or anchor against which an evaluation of current value is only partially adjusted (Tversky and Kahneman 1974, Wright and Anderson 1989). That is, states often "choose" the new payoff from recent changes in the status quo through inaction. This suggests the usefulness of conceiving a "social frame" defined as the perception that participation in the existing social-structural arrangement implies either gains or losses (Berejikian 1992). For example, a state that has recently come to benefit under current conditions will be in a gains frame if it expects that, absent some action on its part, the benefits will continue. Alternatively, a state will be operating under a losses frame if it perceives that, without intervention, recent detrimental changes will continue. The idea is to capture the effect of recent or expected changes in the status quo within terms familiar to prospect theory, thereby expanding the set of cases to which the framework presented below may apply.[8]

Figure 2 represents an example lottery choice set plotted on the S-shaped value function in which status quo and the reference point are identical, both representing current conditions. The choice is between two alternatives to the status quo. By contrast, the idea of a social frame suggests that the location of the status quo can also be distinct from the reference point, such that the payoff from recent (or very highly probable) changes replace the "sure thing," as represented in the standard framing example. Recent (here, beneficial) changes to the status quo payoff are evaluated against the reference point at the graph's origin. The choice then is between passively accepting the benefits from recent changes or attempting to capitalize upon them proactively for further gain.[9]

The argument is that, depending upon the nature of change, a gains or losses frame will exist for at least some period after change has occurred. The speed with which updating takes place, that is, "how long one retains an original point of reference" (Shafir 1992, 316) in assessing the value of recent change, is an important question for prospect theorists but one that is little understood. While a theoretical treatment is beyond my scope here, as a rough guide to empirical research there is some evidence that a losses frame will endure longer than a gains frame. Jervis (1992, 200) suggests that it takes longer to incorporate losses into the reference point than gains:

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[8] Technically, my adaptation of prospect theory requires that the social frame adhere only when the status quo is viewed as distinct from the reference point. When they are one and the same—that is, when the status quo is viewed indifferently—no social frame exists. This restriction is a theoretical necessity. Whether or not it severely delimits the set of interactions to which the model may apply remains an open empirical question. It seems likely, however, that in an anarchic international system fraught with instability, and therefore constant change, the set of cases available for examination under this hypothesis is significant.

[9] An identical logic applies to a losses frame: The choice is between passively accepting the loss or attempting to regain one's former payoff.

> In international politics—and in social life in general... we renormalize for gains much more quickly than we do for losses. We rapidly, if not effortlessly, adjust to good fortune and any improvement in our lives.... Neither individuals nor nations are so accepting of losses, however. We remain unhappy, unreconciled, and often bitter for a prolonged period.

More formally, experiments have demonstrated a related "endowment effect," such that individuals consistently offer to pay less to purchase an item from another than they would be willing to sell it for themselves once it is possessed. That is, decision makers consistently overvalue that which they come to possess, suggesting that individuals (and states) will renormalize for gains more quickly than they will renormalize for losses (Levy 1992, 290). Whatever the duration of the frame, its content is empirically established and not predicted. This is also true under the original formulation of prospect theory except that, as noted above, subjects in the laboratory are confronted with choice sets that any outside observer could easily classify as gains or losses. For present purposes it is assumed that recent objective changes in the status quo are perceived by states, that misperception is minimal, and that objective measures provide a rough guide to the content—in terms of gains or losses—of the social frame.[10] In documenting the social frame, field research must look for evidence of recent compelling objective changes in a heretofore settled status quo that can be easily classified as representing gains or losses.

FRAMING AND GAINS PURSUIT

The next task is to attach the universe of gains concerns to the framing heuristic. Caution is warranted. It has been suggested that at the extremes of "very small probabilities or for catastrophic losses" the framing effect may break down, because it is overwhelmed by the distribution of costs and benefits attendant to the choice set (Levy 1995, 3). For example, it is absurd to suggest that a state would risk a significant probability of destruction to regain, say, 0.1% in GNP. Because clearly defined thresholds have not been identified, we must assume for the moment that most state choices fall somewhere between these extremes. Eventually, if identified, the thresholds would not change the logic of the presentation but would identify with more precision the set of conditions to which it applies.[11]

When applied to state choice, prospect theory makes two general predictions. First, for states that view the status quo as a gain, when confronted with a choice between the status quo and some gamble with a *positive* expected value but also some smaller risk of loss, they will opt for the certainty of the status quo. Second, for states that view the status quo as a loss, when confronted with a choice between the status quo and some gamble with an expected value of *further loss* but also some smaller prospect for improvement, they will opt for the gamble. These general predictions are easily adapted to a state's gains preference. Assume for the moment that a state's preference structure involves only a rank ordering for relative and absolute gains. For states preferring relative gains, framing would predict that: (1) if under the status quo state A loses relative to state B, then A will pursue a strategy to regain the initial distance lost between it and B, even if the expected outcome of this strategy implies further relative decline, so long as it also implies some smaller prospect for relative improvement; and (2) if under the status quo state A gains relative to state B, then A will not pursue a strategy with an expected value of further relative gain but will favor the status quo, if this strategy also implies some smaller prospect for relative loss. Alternatively, assuming that states maximize absolute gains, the predictions are: (1) if under the status quo state A experiences absolute losses, then it will pursue a strategy to regain this loss, even if the expected outcome implies further loss, so long as the strategy also implies some smaller probability that A can regain its former position; and (2) if under the status quo A experiences absolute gains, then it will not pursue an opportunity with an expected value of further gains if this opportunity also implies some smaller prospect of loss.

Traditional theory—realist or liberal—views states as actively pursuing gains whenever the weighted probability of benefits outweighs costs. The logic of framing developed above provides a theoretic scheme that goes beyond traditional theory by explaining when and why states forgo gains maximization (of either type) even when it is to their benefit. But we are still left without an important requirement for a new theory, a prior prediction for state gains maximization. Thus far, like traditional theory, framing predicts through assertion: It depends upon a priori assumptions, such as "given that a state maximizes relative over absolute gains." Without a priori external assertions about goals, framing cannot yet predict by context a state's gains preference.

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[10] Of course, the existence of misperception is not inherently condemning. The possibility of an entirely socially constructed frame is consistent with Kahneman and Tversky's (1982, 171–2) observation that "the reference point is the state to which one has become adapted" and, further, that there are many cases in which "the reference point is determined by events that are only imagined." The conservative approach here springs from the practical concerns of coherence in presentation; it is not intended to rule out the importance of subsequently developing a scheme for assessing the content of subjectively constituted social frames.

[11] Accordingly, for the hypotheses outlined below, it is assumed that the probabilities and payoffs for each choice set do not overwhelm the framing effect. These hypotheses are intended as illustrative. They represent a middle step in the logical construction of an integrated theory of state gains preference, not the integrated theory itself.

Endogenizing state gains preference is achieved through integrating the properties of the S-shaped value function into a theory of leverage. Powell (1991) has demonstrated that under conditions of a prisoner's dilemma, in which states are assumed to maximize absolute gains, there exist plausible scenarios for interaction between states in which an equilibrium solution results in the unequal distribution of gains. A state attends to absolute gains because the relative gains of a rival cannot be altered, and such rival gains have no bearing upon the state's ability to pursue its own (smaller) gains in the next round. That is, to realize any gains the two states must cooperate, and the unequal gains resultant from that cooperation do not represent a threat, nor can they be used to change the incentive structure for the next round of interaction. The logic of Powell's model also argues that once it becomes possible for unequal gains from one round to be used as leverage in the next, states become acutely concerned with their relative position.

Changes in relative position are derivative by nature, as there can be no change in relative position without some change in absolute position. The opposite is not true; states may pursue relative position, but they do so by attempting to alter their own and rivals' absolute gains. For example, to achieve a relative advantage, a state may attempt to improve its own absolute position. Alternatively, it may try to halt its own decline, increase the rate of rival decline, or both. All are strategies intended to secure relative gains. Powell's model implicitly assumes this and recognizes that states are concerned with the relative effect of changes in absolute position. Unfortunately, Powell's example is narrowly developed within the context of a survival game. The advantage gained in one round of interaction is assumed to give the beneficiary a distinct military advantage over its rival in the next; compelling unequal gains always imply increased military threat.[12] A well-recognized distinction exists between "high" and "low" politics, wherein some interactions are fundamental to state survival, others are not. Powell's argument speaks only to high politics, but the principle of leverage should apply across types of interaction. The core issue is the emergence of leverage acquired as a result of the unequal distribution of gains that are the outcome of interaction.

The resolution of the gains debate is to be found, then, in identifying the low politics contexts in which states view changes in their absolute position as posing the threat of leverage, which in turn motivates a concern for relative gains. If the threat of leverage is by definition some function of decline in a state's relative position, then changes in relative position can only result from either slower absolute gains or more rapid absolute losses than those of an adversary. Under framing, there is good reason to believe that states are more threatened by faster losses than by slower gains.[13] In prospect theory, losses have an exaggerated sting because they are measured against the reference point, not a state's overall net asset position. Refer to Figure 1, which presents the S-shaped value function under prospect theory. Because the function for losses is steeper than for gains, losses are overvalued compared to equal gains. This leads states to overvalue that which they lose and overestimate the importance of the relative advantage accrued to a rival as a result of their loss. Because losses hurt more than gains feel good, prospect theory tells us that states will be threatened more by their own absolute decline than by equal rival accomplishments.[14]

Now, as compared to the state's overall position, the actual (absolute) level of loss need not be large to be viewed as serious and, therefore, trigger concern about the potential for leverage. While a loss "might be minor compared to a state's overall position ... because it is evaluated with respect to the current reference point rather than one's new asset position, its effects tend to be evaluated in absolute rather than relative (to total assets) terms" (Levy 1992, 287). This means that "the very fact of a loss is often more important than the magnitude of the loss and that large losses may not be that much worse than smaller ones" (Levy 1992, 287). This suggests that the threshold of decline required to trigger concerns about rival leverage—that is, when states become concerned with relative gains—is quite low, and in some circumstances just the fact of decline is itself sufficient. Loss itself triggers a concern about leverage and therefore about relative gains.

Alternatively, the logic for a gains frame is essentially the opposite of that for losses. The sting of loss means that the threshold of concern is reached very quickly; possibly at the first sense of loss. In the gains frame such an immediate and exaggerated trigger does not exist. Given the exaggerated sensitivity to leverage under a losses frame, if we assume only that states make accurate assessments of the importance of changes in relative position under the gains frame, then we can assert that states are more concerned with relative gains in the losses frame than in the gains frame. Yet, there is good reason to believe that those assessments are not accurate. Recall the endowment effect, whereby actors overvalue that which they come to possess and, therefore, overstate the significance of achieved gains. The effects of this heuristic are not insignificant, as the overvaluation of achieved gains is often quite large (Levy 1997, 89). As a result, states making absolute gains will underestimate an improvement in relative position experienced by a rival who gains more rapidly. Just as there exists an exaggerated attention to losses under a losses frame, in the gains frame there exists a similar exaggeration of achieved gains, and this delays a concern about the increasing leverage of rivals. The state thus is liberated to pursue absolute gains, as Powell suggests, under the illusion of freedom from effective leverage.

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[12] This is not a weakness of Powell's analysis, which was itself partly intended to provoke a discussion about generalizability.

[13] It is assumed below that states make accurate assessments about the absolute gains and losses of rivals. It is reasonable that states are more sensitive to their own circumstances than to those of others. In the construction of a synthetic model, it therefore makes the most sense to begin with states' assessments of their own condition. The next step would be to develop a logic for the process by which states assign value to the gains and losses of potential rivals. This would be at least as complicated as the current endeavor and demands its own dedicated project.

[14] Another implication is that during negotiation states will overvalue concessions made to an adversary "relative to the concessions they receive from the adversary" (Levy 1992, 290). This suggests the realist contention that successful cooperation requires states to receive "proportionate advantages in return" for concessions is misplaced (Grieco 1990, 47). Levy's example demonstrates that equal concessions leave both parties feeling a loss. From a realist's point of view, such a condition should never lead to cooperative outcomes.

Framing thus allows for a synthesis of realist and liberal assumptions. Rather than assert universalistic goals across contexts, the picture that emerges under framing is that states are prudent maximizers. They pursue absolute gains whenever possible, but they also are concerned with external threats to sovereignty and therefore act to shield themselves against the possibility of leverage. While benefiting under the status quo, states opt for conservative policies, but they are quick to act if this beneficial structural arrangement is threatened. Alternatively, while suffering under the status quo, states are willing to incur greater risk in an attempt to return to safety. Put succinctly, *states in the gains frame pursue absolute gains and are risk averse, while states in the losses frame pursue relative gains and are risk acceptant.*

THE STATUS QUO AND NEGOTIATION

Prospect theory also provides insight into the dynamics of negotiation. It predicts that states in a losses frame are more willing to enter into serious negotiations than are states in a gains frame. This prediction derives from the finding under prospect theory that decision makers treat risk differently in the two frames.

In low politics disputes are often resolved through negotiated agreement.[15] Meaningful participation in negotiations requires committing to a process with the intent of improving one's condition but without knowing the ultimate outcome. Indeed, often the most difficult hurdle in achieving a settlement is getting the aggrieved parties to agree to the principle of negotiation. Because there are significant costs for arbitrarily exiting negotiations once committed to them, states often prefer an informal prelude. This is an attempt to manage both the uncertainty inherent in negotiation and the costs of exiting.

> That is, prenegotiation is attractive to states, because it promises lower exit costs than formal negotiation does. . . . The option to withdraw remains open, and leaders can exercise that option before domestic political interests have had the opportunity to organize and mobilize, either in behalf or against negotiation. Prenegotiation is usually a less public process than is negotiation and interested outsiders have less access to critical information. [Prenegotiation] is better insulated from political interests and, in this sense too, leaders can acquire critical information at lower cost. The political costs of information are less at the outset of prenegotiation than in negotiation (Stein 1989, 247).[16]

The exit costs associated with abandoning formal negotiations midstream, combined with the fact that the eventual outcome is unknown, suggests that the decision to enter into the process is a gamble not taken lightly. Yet, if the only alternative to negotiation is costly conflict (e.g., the Cuban missile crisis), then negotiation is not such a risky strategy.[17] Therefore, the dynamics described above do not apply in those scenarios but are an accurate representation when the alternative to negotiation is the continuance of current conditions. Often this means that the status quo settlement is consistent with the distribution of power among the parties. Negotiation then represents commitment to a process, whose outcome is uncertain with the intent of achieving gains that cannot be obtained unilaterally.

Given the framing effect, we would expect that states facing losses are more apt to gamble than states benefiting under the status quo and are more willing to accept risk than states attempting to alter currently beneficial conditions to their further benefit. Framing therefore predicts that states facing loss are more likely to risk negotiation than are states profiting from the status quo, even if the latter see some possibility for further improvement through negotiation. Yet, if entering into negotiations is considered a gamble, then we would expect very few agreements. Aggrieved states, being risk acceptant, would constantly call for resolution, while beneficiary states, being risk averse, would simply ignore them. Nevertheless, binding treaties are negotiated all the time. Recall from the discussion of renormalization that losses are more slowly incorporated into an updated reference point than gains. This suggests the possibility of states with opposing assessments of current conditions both believing that they are defending the status quo against change. For example, Levy considers the following scenario:

> States might identify different reference points to frame their respective decisions, and this might lead them to both perceive that they are defending the status quo. Consider a situation in which state A has just made a tangible gain at state B's expense, say through the seizure of territory or control over a vital operational area. The endowment effect suggests that A will accommodate its gains much more quickly than B will accommodate its loss. Consequently, B will attempt to recover its losses and restore the old status quo, and A will attempt to maintain the new status quo against B's encroachments. Each will accept larger-than-normal risks in order to maintain its version of the status quo (Levy 1992, 288).

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[15] Of course, issues concerning high politics are also often resolved through negotiation. The following argument applies here as well.

[16] In the terms of a two-level game, the process of negotiations alters what Putnam (1988) has called the domestic win set: the size and content of domestically acceptable solutions to an international conflict. In addition, it may be that the reputational costs of being seen as an unreliable negotiating partner in subsequent interactions also inhibits states from unthinkingly abandoning negotiations once committed to them.

[17] The author is indebted to an anonymous reviewer for both clarification on this point and the example.

Levy's stylized example highlights the analytic importance of distinguishing between the social frame and the reference point (noted above). While one state benefits and another suffers under current conditions, both are defending the "status quo" and as a result are more likely to accept the risks inherent in committing to negotiation. For state A, the status quo and the reference point are the same. Both represent current empirical conditions. For state B they are distinct. Therefore, we would expect A to adopt a risk-acceptant decision rule only if it views B's threats to recoup its losses as viable. If not, then the status quo is not threatened, and A will be risk averse, opting out of any attempt to secure a negotiated settlement. The key point is that a viable challenge to the status quo makes A believe that if it does nothing the status quo will change to its detriment, and this new threat to the updated status quo makes A risk acceptant.[18]

SUMMARY

The set of hypotheses that define states as prudent maximizers challenges directly the notion that the intentions of states derive from properties inherent to the international system and are immune to the influences of context. It also represents a retreat from the grand or universal claims of traditional theory to a more modest middle-level approach. Rather than aspiring to explain all changes in the international political topography from within a single perspective, the approach here takes such changes—defined as changes in the social frame—as given; it is concerned not with their genesis but with their effect upon state choice. Prospect theory does not predict beforehand the reference point for a state decision.[19] Instead, the content of the social frame is empirically established through a detailed examination of the relevant issue area.

It is important that this also defines a new dimension to the application of power: manipulation of the social frame. The idea that power is useful in circumscribing a rival's choice set or getting a rival to negotiate in good faith has a venerable history. But the assumption traditionally has been that the targeted state's goals remain unchanged in the face of power, even if the current distribution of power denies the possibility of achieving them. The notion of prudent maximizers developed here adds to this the idea that an unintended consequence of the successful application of power—to the extent that it alters the social frame—is the possible alteration of the policy goals of the targeted state.

An empirical assessment of the contention that states are prudent maximizers entails two separate endeavors. First, we must determine the social frame under which a state is operating. Second, we must fix the goals pursued for the duration of that frame.[20] Determining the content of the social frame for several cases requires extensive historical documentation and thus a rather detailed discussion which space does not permit. Also, there is no logic by which to select a single case in which both realism and liberalism can be expected to succeed and a synthetic model fail, because the predictions about the content of state preferences derived from realism and liberalism are mutually exclusive. As an alternative, a few cases could be chosen with an eye to variation in the social frame, and one could then compare the content of the frame against the states' observed gains policy. Yet, this would allow both realists and liberals to claim that the small number of cases biases against their perspective, whereas a large number would demonstrate general patterns of relative or absolute gains pursuit.[21]

A more attractive option is to select a case that neither camp can claim as its own. Because both camps treat state preferences as fixed across contexts, this means choosing a case in which a state shifts its gains pursuit within a single issue area, ideally during a single negotiation. While not a comprehensive test, explaining such behavior is important in that current theory must treat it as anomalous. Success here is also strong evidence that a fruitful synthesis of realist and liberal perspectives is at least possible. Empirically, this option requires a detailed discussion of the content of the original social frame and the type of gains originally pursued; how and why the frame shifted; and whether, with the new frame, the state's gains preference changed accordingly.

THE EUROPEAN COMMUNITY AND THE MONTREAL PROTOCOL

Previous sections developed the argument that states pursue absolute gains in a gains frame and relative gains in a losses frame. This prediction is measured against the behavior of the EC in the negotiations leading up to the 1987 Montreal Protocol, which limited the production of ozone-destroying CFCs in signatory countries. The protocol was the result of more than a decade of international negotiation. It was first hypothesized in 1974 that the release of CFCs into the atmosphere might harm earth's protective ozone shield (Molina and Rowland 1974). The prospect of ecological collapse spurred an initial round of international talks. Shortly thereafter, formal discussions began under the auspices of the United Nations Environmental Program.

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[18] Such a threat need not necessarily come from an adversary/rival/competitor. Theoretically, any external threat to A's newly updated status quo would trigger risk acceptance. This example also reinforces the point made above that the content of the social frame must be established empirically.

[19] For further discussion, see Stein and Pauly 1993, 16–9.

[20] Proponents and critics of the application of framing to international outcomes caution against misuse (Jervis 1992, Levy 1992). The concern here is that a misuse of prospect theory lends itself to a framing version of revealed preference and therefore teeters on the tautological. In rational choice this is a danger because independent and dependent variables involve identical empirical realms: The preference for *X* over *Y* is exhibited through the choice of *X*. Under the framework developed here the danger of tautology is less troubling, because the independent and dependent variables are conceptually and empirically distinct. The former is a state's understanding of the status quo, and the latter its gains preference. Because there is little if any overlap, the critical reader must ask whether the social frame asserted is empirically established. If the answer is yes, then the concern about tautology is misplaced.

[21] This sort of preponderance of evidence is a theoretical possibility under the current constitution of the gains debate, though I am not aware of a self-classified realist or liberal who has ever suggested it.

The Montreal Protocol cannot be considered a critical case study which alone "disproves" both realism and liberalism. Instead, EC participation in the construction of the protocol represents an empirical puzzle from the perspective of traditional theory and as such deserves examination. Any model attempting to explain the EC's behavior must account for both the change from absolute to relative gains pursuit and the decision to enter into serious negotiations in early 1987 after several years of opting out. Accordingly, the focus below is not on final treaty details but on the evolving position of the EC.

EC participation can be separated into two stages. The first began in the early 1980s and concluded with the Vienna Convention, which did little to protect the ozone layer but called for further talks. The second stage began shortly after Vienna and concluded in Montreal in 1987 with a binding protocol. Throughout, the content of the frame under which the EC operated is attributable to identifiable and objective changes in the status quo that were beyond the control of EC member states. During the first stage, CFC producers in the EC enjoyed dramatic increases in production and global market share due to a unilateral U.S. ban on the domestic use of CFCs as aerosol propellants. The continual benefits flowing to the EC thereafter placed it in a gains frame. During the second stage, several features of the international political topography changed, threatening not only the recent gains but also the entire CFC industry. Here, the EC operated under a losses frame. The following description documents, first, that the social frame for the EC switched from gains to losses in the period between the Vienna and Montreal and, second, that with this change the EC's policy shifted from absolute gains pursuit and risk avoidance to relative gains pursuit and risk acceptance.

THE EUROPEAN COMMUNITY NEGOTIATES A PROTOCOL: THE EC AS A "STATE"

One is immediately confronted with the argument that it is not always theoretically defensible to treat the EC as a unified negotiating entity. I would argue to the contrary. In the deliberations leading up to the Montreal Protocol, a combination of domestic politics, internal EC power relations, and EC decision-making procedures makes such an assertion both historically accurate and theoretically useful.

The authority of the EC to negotiate binding treaties with other countries derived gradually and under a series of European Court of Justice rulings.[22] In the EC legislature, it is the European Commission that develops and proposes legislation for ratification by the European Council. Where the EC has "competence," the commission is charged to negotiate on behalf of the EC, consistent with a council mandate, which must be unanimously approved. The commission has continually attempted to expand its authority by claiming the competence to negotiate broader issues, while the council, composed of national ministers, is often more protective of member-state interests.[23] Prior to the Single European Act, which gave the EC competence in environmental affairs, it was not clear that the commission had authority to negotiate a CFC ban on behalf of the members. Yet, because CFC regulations would directly affect the relations of production across member states, a uniform EC policy was mandated (Jachtenfuchs 1990, 262). Accordingly, a unanimously approved council mandate gave the commission authority to enter into negotiations.

Under a unanimous council decision rule, any member state obtains effective veto power over the development of EC policy. In the context of the Montreal negotiations,

> states strongly influenced by their respective chemical industries could force others [in the community] to join their position. This meant harmonization at the lowest common denominator. In the last resort, it was the successful lobbying of two chemical companies at the national, and not at the European level which determined the international position of the Community (Jachtenfuchs 1990, 275).

Thus, Britain and France on behalf of domestic chemical producers, most notably France's Atochem and Britain's Imperial Chemical Industries, were able to dominate EC policies and negotiating positions. Indeed, industry officials often sat as representatives at the formal negotiations, with their U.S. counterparts relegated to unofficial status. The arrangement meant that "industry representatives substituted themselves [for] politicians in the decision making" for the EC (Benedick 1991, 34). This industry influence, when combined with the commission's weak position within the EC and a unanimous council decision rule, suggests that for all practical purposes and throughout most of the negotiations there was in fact a single EC position, and it closely reflected the interests of large CFC producers in Britain and France.[24]

ESTABLISHING THE SOCIAL FRAME PRIOR TO VIENNA: GAINS

U.S. reaction to the initial 1974 ozone-depletion hypothesis led to a ban on the domestic nonessential uses of CFC propellants by 1978. This unilateral move allowed EC producers to realize both relative and absolute gains over the United States. A 95% decrease in U.S. production of CFCs for aerosols eventually allowed the Europeans to become the largest producer of CFCs.[25] The EC also became the single largest exporter and was the only major international supplier. By 1985, EC exports had risen 43% over 1976 levels, accounting for fully 30% of total EC production (Commission of the European Communities 1985). CFCs were also important to the aggregate EC economy, as they accounted for 50% more of GNP in the EC than they did in the United States.

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[22] For a discussion of the initial evolution of external negotiating authority, see Haigh 1991. The description here reflects the EC decision-making procedures during the Montreal Protocol negotiations. These procedures continue to evolve.

[23] Where the EC has no exclusive competence, the member states act independently (Mastellone 1981, 105–17).

[24] The fact that British and French producers dominated the EC position is not contested in the literature on the Montreal Protocol. The purpose of this discussion is to reinforce the point, unearth the institutional dynamics that permitted this situation, and establish the EC as a unified decision maker with a single coherent policy for the purposes of this case.

The market forecast for CFC demand was even more promising. Many nations were just entering a level of economic development in which CFCs would be needed for domestic product manufacturing in large quantities; these included, by order of potential use, China, India, Brazil, Saudi Arabia, South Korea, Indonesia, Nigeria, Mexico, Turkey, and Argentina (Kohler 1987). Along with requiring CFCs for production, developing countries were projected to increase imports of goods that incorporated CFC. These forecasts suggested that the eventual CFC demand in developing countries would be significant. For example, annual growth rates in consumption for China, India, and Brazil were estimated at 12%, 15%, and 8%, respectively, from a 1986 baseline (Munasinghe and King 1991). What is more important, EC production capacity in 1986 was 30–40% greater than actual production levels. This meant that EC producers were uniquely positioned to capitalize efficiently on the growing market.

In addition, the perception within the EC was that it had responded adequately to the ozone threat, and no further actions were required. After the U.S. ban in 1978, the United States attempted bilateral talks to compel the EC to enact similar restrictions. The EC response in 1980 was a decision to reduce CFC aerosol use by 30% from 1976 levels by 1982 (European Communities 1987). The move was largely symbolic, as "this level had already been reached at the time the decision was taken" (Jachtenfuchs 1990, 213), but it allowed the EC to argue that it was not blind to the ozone issue and had taken measures commensurate with the current level of scientific understanding (Commission of the European Community 1986).

Thus the EC had, by its view, dealt with the ozone issue in such a way as not to harm its status as the dominant exporter or its positioning to capitalize on future growth in demand. In this brief period leading up to the Vienna Convention, EC producing states enjoyed a considerable windfall in the CFC market. These gains were the natural consequence of U.S. unilateral action and the rapid increase in market demand driven largely by developing countries.

EC POLICY AT VIENNA: ABSOLUTE GAINS AND RISK AVOIDANCE

The United Nations Environmental Program (UNEP) placed the threat to the ozone layer at the top of its agenda in 1981. Multilateral negotiations began at Vienna in 1982 under UNEP auspices with the intent of negotiating a binding protocol on the reduction of CFC emissions into the stratosphere. By 1984 the U.S. position, backed by the other members of the Toronto Group, was that a ban on the nonessential use of CFCs as aerosol propellants was the quickest way to address ozone depletion and that the U.S. experience had proved this approach to be the least disruptive economically.[26] By contrast, EC representatives downplayed the need for a treaty, arguing that the science on ozone depletion was incomplete, no effective substitutes existed or were imminent, and hasty action based on highly questionable science would be economically costly.[27] U.S. delegates decried the EC position, attributing it to the influence of CFC producers, whose aim seemed to be "to avoid for as long as possible the costs of switching to alternative products" (Benedick 1991, 33).

This clash between the Toronto Group and the EC eventually deadlocked the negotiations. Without compromise, it quickly became clear that no binding agreement could be achieved at Vienna by the 1985 deadline. The negotiations quickly degenerated into a discussion of side issues and an attempt to get a nonbinding convention that would include a statement on the need for increased research and, if possible, further talks to conclude in Montreal (Jachtenfuchs 1990, Sands 1985). The EC did, in fact, offer a two-pronged counterproposal. It suggested a 30% reduction in the use of CFCs in aerosols from 1976 levels and a freeze in production capacity on CFCs 11 and 12 (Lammers 1988, 228–9). The EC argued that a capacity cap was the only effective way to halt the growth of CFC emissions in both aerosol and nonaerosol uses, and it correctly noted that the U.S. aerosol ban left the increasing rate of growth in nonaerosol sectors unchecked. Furthermore, the EC noted, there were relatively few CFC producers but thousands of manufacturing consumers, and a capacity cap was therefore significantly easier to implement.

Still, participants to the negotiations seemed bemused by the EC's position. Indeed, from the perspective of realism, the EC intransigence is difficult to grasp. Throughout, the EC remained obstinate, sticking exactly to this original proposal. If, as realists assert, states concern themselves only with relative gains, then the early U.S. position provided an opportunity for the EC to guarantee growth in market share. A capacity cap would create a production ceiling for those states, such as the United States, which were producing at capacity and lock out altogether those states which until then had not produced CFCs. States in either condition would be prevented from increasing their share of the international market in CFCs. By contrast, EC production facilities were operating at about 65% of maximum. Given the projected rate of growth in demand, the cap would actually allow the EC to continue to increase production at least through 2000.[28] In other words, a freeze would fix the production level of other states while allowing the EC to increase its own as the market grew, thereby assuring its current position as the dominant global supplier and making it the only supplier able to meet future increases in demand.

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[25] For a more detailed discussion of the effect of unilateral U.S. action, see Benedick 1991, 21–7, and Dotto and Schiff 1978, 148.

[26] Nonessential was largely defined as those uses which do not affect national security.

[27] For summaries of the early EC position, see, for example, Szell 1985 and Guest 1985.

The pursuit of relative gains therefore would require a counterproposal whereby the EC would accept some cutbacks in aerosol use in exchange for a production capacity freeze. A reduction of CFCs as aerosol propellants would imply a loss, in absolute terms, for domestic producers of CFCs for aerosol products. Yet, the U.S. experience had demonstrated that the effect on the overall economy—and on the myriad manufacturers who used CFCs—was minimal at worst. Substitute propellants proved to be inexpensive and safe, and the increased cost to the consumer of switching over was negligible (UNEP 1986). On balance, the best data suggested that the short-term loss to the EC in absolute terms, even if it had to eliminate CFCs as aerosol propellants, would be small.[29] By contrast, as noted above, the potential relative gains of a capacity cap were significant.

This was an obtainable agreement for the EC. While a counterproposal securing relative gains clearly would have favored EC interests, in offering a compromise the EC would not be in such a weak position as it might first appear. The United States did not argue against a capacity cap on the ground that it was unfair to its domestic producers; they were operating at capacity, so the decision to compete for a greater international market share would have been an expensive one. Rather, the United States contended that the proposed EC cap would allow continued growth in CFC use and thus not effectively halt ozone depletion. In addition, it was quickly recognized by all participants that the EC proposal had the distinct advantage of placing a lid on all other nonaerosol uses of CFCs as well. Because these comprised the fastest growing sectors of the CFC market, the EC proposal could honestly claim to be more comprehensive. Finally, throughout the Vienna negotiations, the only consistent U.S. demand was for an aerosol ban.

Instead, the EC was quite happy to scuttle the attempt to get a binding protocol by adopting the position that any cuts to current use levels as would be required by an aerosol ban were unacceptable. While the European Commission was formally a participant to the negotiations, the council mandate given to the commission allowed for no departure from its initial restrictive stance of a 30% reduction in aerosols from a 1986 baseline and a production capacity cap (European Communities 1982). Under this restrictive directive it "rather quickly became clear that the Community was not even prepared to negotiate on any form of reduction of CFC production or use at that time," because the council decision "left no room for CFC reductions in the Vienna discussions" (Jachtenfuchs 1990, 263). This effectively shackled the commission, preventing it from negotiating in any meaningful sense of the term. In fact, the mandate denotes a decision by the EC to opt out of the negotiations altogether, a position held throughout the initial rounds of Vienna talks. The intent was not lost upon the U.S. delegation:

> For its part consistent with its "status quo" decision, the EC Commission declared that it had no mandate to negotiate anything other than a cap on production capacity. EC and UK representatives then attempted to postpone the next round from the previously scheduled February 1987 date until April. When the plenary rejected this tactic, the heads of the UK and EC Commission delegations departed from Geneva prematurely (Benedick 1991, 70).

The strong EC refusal to negotiate reflects its desire both to avoid the risk inherent in engaging in meaningful talks and to protect absolute gains. Both goals are consistent with the description of states as prudent maximizers developed earlier. Given recent gains in production, a unique market position, and the projections of continued increased demand, the EC was clearly operating under a gains frame. As argued above, the decision to commit to negotiation is viewed by the participants as a gamble in which the outcome is unknown. Prospect theory predicts that, when confronted with the choice between the (beneficial) status quo and a gamble for increased relative gains that also would entail some risk to absolute production levels, the EC would forgo an opportunity to improve its relative position in favor of the status quo. Remaining immobile and effectively opting out of the negotiations shielded CFC producers against U.S. attempts to curtail the short-term gains in production levels achieved following unilateral U.S. restrictions. While the EC no doubt would have preferred both relative and absolute gains, when forced to choose it elected to protect recent absolute gains in production over relative gains in market share. This supports the contention that states will not actively pursue relative gains, as realism predicts, if in such a pursuit they must risk disturbing a beneficial status quo. This appears true even when the potential costs are likely to be small relative to the potential benefits.

ESTABLISHING THE SOCIAL FRAME AFTER VIENNA: LOSSES

While no binding protocol was concluded in Vienna, participants did agree to continue discussions. After a brief respite, the parties were reconvened. During the period immediately following Vienna several aspects of the political topography changed, as did the implications of the status quo for the EC. The changes included (1) a revised incentive structure for the United States resulting from domestic political shifts; (2) an emerging scientific consensus on the link of CFC to ozone depletion; and (3) a reversal in the position of

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[28] For a detailed presentation of European production, consumption, and capacity statistics, see Commission of the European Community 1986.

[29] This cost, however small, would be further reduced to the extent that the EC could negotiate a reduction short of a total ban on aerosol uses.

U.S. producers of CFCs on the desirability of a binding protocol. As a result, the social frame for the EC changed from gains to losses. To the extent that the above discussion establishes the EC as protective of absolute gains, it is also possible to interpret EC actions as simply consistent with the predictions under liberalism. Yet, with the shift in the social frame from gains to losses, the EC quickly abandoned its defense of production levels and chased relative position.

U.S. Unilateral Action

In May 1986 the Natural Resources Defense Council sued the Environmental Protection Agency (EPA) under section 157(b) of the Clean Air Act. This section charges the EPA to issue "regulations for the control of any substance, practice, process or activity . . . [that] may reasonably be anticipated to affect the stratosphere, especially the ozone in the stratosphere" (Clean Air Act 1982). The suit alleged that the EPA was not carrying out its duties under the act. The district court agreed. The EPA was ordered to publish a set of proposed regulations by May 1, 1987, and submit final regulations for CFC restrictions by late 1987. Thus, the United States was again moving to restrict CFC emissions unilaterally.[30] Initially, since this seemed a repeat of earlier U.S. unilateral action on aerosols, it appeared to be a victory for the EC. U.S. product manufacturers who used CFCs would be required to integrate more expensive substitutes into their processes.[31] Also, U.S. producers of CFCs had cultivated very few clients outside the United States to whom they could send surplus production. Instead, the ruling provided the impetus for a spate of preemptive congressional attempts to regulate CFCs.[32] Pushed by the court-mandated deadline, the introduction of ozone protection bills reflected a concern by Congress that international negotiations would ultimately prove unproductive, leaving the United States in a position of repeating the mistake of unilaterally imposing a restriction that put U.S. producers at a disadvantage.

The primary target of these bills was not the EPA but what Congress viewed as a reluctant international community.[33] With the court mandated EPA deadline approaching, Congress attempted either to force the pace of negotiations or to ensure that the United States would not be disadvantaged if they failed. For example, upon introducing the Stratospheric Ozone and Climate Protection Act, Senator Chaffee argued: "My preference is still that this global problem be dealt with by way of international agreement. For this reason, we will not press for consideration until it becomes obvious that the ongoing negotiations will not produce an acceptable agreement" (Congressional Record 1987). He then insisted: "Call it what you will, countries that wish to trade with us will have to abide by our rules for protecting the environment" (ibid.). Indeed, all the bills introduced at this time had strict provisions to insure that America's trading partners played by its rules. In addition to a unilateral phase-out of CFCs and restrictions on imports in bulk, these bills all proposed in some way to halt the import of all goods that used CFCs in their manufacture.[34]

To make the intent of the proposed domestic legislation clear, the U.S. delegation to the protocol negotiations took care to draw attention to these efforts. The delegation went so far as to suggest that the forces desiring strong action were beyond control, and if no effective multilateral pact were signed, then domestic legislation would be inevitable. That is, "U.S. negotiators made certain that the implications of this threat were not lost on foreign governments, pointing out that there might be a price to pay for not joining in meaningful efforts to protect the ozone layer" (Benedick 1991, 29). In a more public forum, during an opening plenary statement, the head of the U.S. delegation publicly noted the EC's intransigence: "People are beginning to say that some parties to these negotiations seem to be viewing the ozone issue mainly in terms of narrow self-interest . . . other producing companies—many of them actually here representing governments on official delegations . . . tell us to wait" (Benedick 1987, 13). He then threatened: "Distinguished delegates, I cannot emphasize too strongly the high degree of interest in the United States in the outcome of these negotiations . . . Last week, a bipartisan coalition introduced a concurrent resolution" intended to eliminate the use of CFCs in the United States and "stop making them and refuse to import them" (p. 14).

The effect of these legislative threats went far toward changing the implications of the status quo for the EC. The message was that if nothing was accomplished at the international level, the U.S. might act unilaterally: "The experience in Geneva and Vienna shows that even mere talk of such a restriction gets the attention of other governments" (Wirth 1987, 259). Producing states also thought the United States was serious. One French delegate noted informally, after the threat of unilateral action was made explicit: "When your country proposed the phase-out in Geneva, we did not realize that the U.S. took it seriously. Now we do" (U.S. Congress 1987a, 257). In addition, while the unanimity rule still existed within the European Council, various EC members now had a stake in the negotiations and no longer deferred to producing states.

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[30] During this period the United States also changed its position at the negotiations. It now held that a (virtually complete) ban on the production of CFCs was the only way to protect the ozone layer. The reasons for the shift are not discussed here. For present purposes it need only be noted that this stronger position was partially due to the need to secure an international treaty consistent with impending domestic regulation. The change in position also was a function of diplomatic posturing, as the U.S. delegation never intended (or hoped) to ban CFCs completely.

[31] Unlike substitutes for aerosols, which were available and relatively easy to find, substitutes for the myriad other uses for CFCs were not yet developed and were expected to be more expensive.

[32] See, for example, U.S. Congress 1987b, c, d, and e.

[33] Some in Congress were also displeased with the slow pace at which the EPA had moved in addressing this issue. See U.S. Congress 1987a.

[34] A partial list included automobiles, refrigerators, air conditioners, packaging, insulation, Halon fire extinguishers, furniture, carpeting, footwear, clothing, and electronics.

It would be easy to overestimate the importance of this single factor. It was not at all clear that the United States would have enacted a total ban on the wide array of products that require CFCs in their manufacture, as the value of such goods was far greater than the receipts from the production of CFCs in bulk. There also were significant forces in Congress who, consistent with a long-standing free trade policy, were unwilling to abandon open trade for ozone protection or to accept the significant costs associated with likely retaliatory EC trade legislation. Finally, congressional bills are often introduced and never acted upon, intended for constituent consumption only. The point is that the United States effectively signaled to the international community and the EC that it was likely to act (in some fashion) on CFCs and was unlikely to bear the burden of such action unilaterally.

Scientific Consensus

While it fell short of a binding protocol, the Vienna Convention had established a mechanism for international cooperation and coordination of ozone research. Whereas scientists had been working independently, resources and data now could be shared, and suggestive findings could be quickly verified. This provision was intended to provide the institutional infrastructure to engender consensus on the effect of CFCs. This cooperation meant that "the science converged from 1985, [and that] the authoritative international panel reports progressively delimited the range of possible disagreements that could credibly be attributed to different views of science" (Parson 1992, 28).

A primary component of the EC's blocking strategy was to argue that the science of ozone depletion was at best inconclusive. Initially there was significant discord within the scientific community as to the nature of the relationship among ozone, CFCs, and atmospheric dynamics.[35] The uncertainty anchored the EC's assertion that, while it was greatly concerned with the well-being of the ozone layer, given current science, all that was required was a limited cap on future emissions. By 1987, however, the scientific topography had changed, and there emerged a general, although not uniform, consensus on the notion that CFCs caused ozone depletion.

In 1984 a study was launched to "provide governments around the world with the best scientific information currently available" on the effect of CFCs on the ozone layer (Parson 1992, 4). In July 1986 the results were published (NASA 1986). Because this report had broad international financial support, its conclusions carried significant weight (Haas 1992, 35). It found that the threat to the ozone layer was authentic and that CFCs deserved attention as the major culprit. It also noted that CFC molecules were potentially several thousand times more potent than carbon dioxide as a greenhouse gas. The report was followed by a series of UNEP and EPA workshops. Under the newfound institutional cooperation, predictions from the latest atmospheric models were quickly compiled and disseminated. The general conclusion was that even if CFC emissions were frozen at current levels, ozone depletion would continue.[36]

The effect of the scientific consensus was felt immediately. It is important to stress that at this point (1986–87) there was still no hard empirical evidence outside the laboratory to support the hypothesis. The various scenarios for depletion rates were based upon elaborate computer models. Competing models incorporated different assumptions about the rate of CFC emissions, the transfer rate into the stratosphere, atmospheric dynamics, and the influence of other trace chemicals (Roan 1989). Nevertheless, while predictions differed somewhat, agreement coalesced around the notion of depletion itself as well as the predicted rate of depletion under current levels of CFC consumption. At a protocol workshop in Leesberg, Virginia, a general understanding emerged among virtually all the participants except the EC (which still trumpeted scientific uncertainty) that if current estimates proved correct some form of international regime would be the only effective method to halt what was now recognized as a legitimate threat to the ozone layer. This consensus was also part of the shift in the status quo for the EC. While a strong international protocol was by no means assured, the perception that the EC would be operating under some form of CFC regime or trade strictures, whether or not it participated, increased significantly.

Industry Shift

Shortly after the U.S. unilateral aerosol ban, CFC producers in the United States formed the Alliance for Responsible CFC Policy, which was very successful in opposing further national restrictions on CFC production. Then, unexpectedly, U.S. producers broke ranks with their European counterparts and came out in support of an international agreement. The fact that the push for regulations counter to industrial interests came from a Republican administration with impeccable antiregulation credentials troubled EC representatives. By now a bit desperate, EC countries "distrusted the motives behind the U.S. position" and suspected that any strong regulatory proposal from the Reagan administration could only mean that "U.S. industry had secretly developed CFC substitutes" (Litfin 1992, 267). Because it could not see any rational reason for the shift in position of U.S. industry, the EC incorrectly attributed it to a secret plan to flood the international market with cheap substitutes as soon as a protocol was completed.[37]

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[35] For a detailed historical account of the evolution of scientific consensus on the harmful effects of CFCs, see Roan 1989. The primary focus of her investigation is the history of scientific disagreement up to an Antarctic expedition just after the signing of the Montreal Protocol.

[36] For example, see Environmental Protection Agency and United Nations Environmental Program 1986.

The political implications of this industry about-face were significant. Most important, the public explanation was that the science of ozone depletion was compelling enough to justify some limited international action. Richard Barnett, head of the Alliance for Responsible CFC Policy, proclaimed that "all of the computer models calculate that large future growth in CFC emissions may contribute to significant ozone depletion in the latter half of the next century" (Barnett 1987, 342).[38] This stance from an industry with a seemingly clear interest in preventing restrictions undercut the credibility of the EC position:

> By announcing acceptance of the need for at least a freeze, Dupont undermined the ... argument that the scientific evidence did not justify any action and at the same time bolstered the ecological epistemic community's claim that strong CFC control measures were necessary. The community's claim gained support and eventually prevailed not because of improved explanatory power but because the alternative position was undercut (Haas 1992, 37).

The general view was that "this policy change, which broke industry's transatlantic united front practically on the eve of the negotiations, was greeted with consternation in Europe" (Benedick 1991, 32). The U.S. negotiating team could claim that it had domestic industry "on board" and in agreement on the need for strict regulations. While this overstated the U.S. CFC industry's support, it did indicate that meaningful domestic opposition to a binding international protocol was significantly weakened. Indeed, even those in Congress who offered unilateral legislation, in some cases as an *incentive* to get the EC to negotiate seriously, claimed industry support. Thus, the American position was significantly stronger than in Vienna, as was the perception that U.S. domestic regulation was likely should negotiations fail.

REVISED EC POLICY: RELATIVE GAINS AND RISK ACCEPTANCE

These changes constituted a viable challenge to the benefits enjoyed by the EC under the status quo. When evaluated against the recently achieved gains, the status quo now represented the possibility of significant losses for the EC. There was the possibility of multilateral action without the EC as participant, U.S. unilateral action, or both. Prospect theory suggests that the introduction of this viable threat to the status quo would make the EC risk acceptant in the pursuit of relative gains. Indeed, the EC quickly altered its posture at the Montreal talks. For the first time it gambled and fully committed to negotiations, abandoning its concern for absolute gains to protect its relative position as the dominant exporter to global markets.

New Negotiating Autonomy

In late 1986 the European Commission reassessed the most recent scientific conclusions regarding the threat CFCs posed to the ozone layer, the cost and effectiveness of various control schemes, and the status of the international negotiations. The report was submitted to the European Council along with a renewal for a negotiating mandate. While reaffirming its position, the commission also deftly presented the council with an opportunity to alter the EC posture at the next round of negotiations. Lauding past EC actions to protect the ozone layer, the commission cautiously suggested that "modification in detail may be required in the context of a regulatory system" (Commission of the European Community 1986, 9). The proposed "Directives for the Negotiations" annexed to the document suggested that the EC position should

> provide for appropriate global strategies and policies for the control of emissions of chlorofluorocarbons, which would be consistent with the broad framework of existing policies developed within the Community, but should recognize that the changing state of scientific knowledge, and the positions of other parties to the negotiations, may require some changes to the details of these policies (Commission of the European Community 1986, annex p. 6).

The existing mandate had authorized the commission to negotiate a treaty under the stricture that "no modification, even of details, in existing Community policies to control CFCs must be made without prior approval from the Council" (Jachtenfuchs 1990, 265). As noted above, this effectively strangled the negotiations. Accepting the notion that "some changes to the details" would be required to achieve an international agreement represented a meaningful alteration. Not only did the commission's rationale for this change derive from an acknowledgement of the importance of achieving a treaty, but also the granting of new negotiating latitude (indeed, any latitude at all) would signal a willingness on the council's part to allow the commission to secure a treaty.

The commission's request was quietly "settled by an informal meeting of the Environment Council before negotiations resumed. The Council ... simply gave the Commission more room for interpretation" within the original mandate (Jachtenfuchs 1990, 265). The strategy was rational. Intended to minimize the risk inherent in entering into meaningful negotiations for the first time, a more relaxed interpretation permitted the commission to negotiate minor issues, such as timetables for implementation and verification procedures, but not move substantially away from the existing EC position.

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[37] See, for example, Benedick 1991, 33, and Litfin 1994, 234. There was no such plan, as no substitute had yet been developed. In conjunction with this argument, the EC also complained throughout that it was ten or fifteen years behind the United States in developing substitutes. See Lewis 1987, E26.

[38] While a full explanation of this change will not be given here, it is important to note that industry's new position was motivated by enlightened self-interest rather than scientific persuasion. U.S. domestic producers faced a spate of local and state ballot initiatives intended to eliminate the use of CFCs in these localities. The result would be a patchwork of legislation requiring different standards across the country. In addition, the alliance faced the prospect of unilateral U.S. action, which it vehemently opposed. Still, the alliance disseminated considerable information regarding its rationale for shifting position, all of which expressed confidence in the science of ozone depletion and the need for international action.

Relative Gains

Once committed to negotiations, however, it quickly became clear that a more realistic posture was required. The emerging scientific consensus linking CFCs to ozone depletion, combined with new findings that emissions had drastically increased in the 1980s, allowed proponents of a protocol to argue that at the very least a freeze was needed while more conclusive studies were conducted. In order to be viewed as a meaningful participant whose proposals would be taken seriously, the EC abandoned its position on absolute gains. Dropped entirely was its defense of CFCs as aerosol propellants. What in previous rounds had been nonnegotiable was no longer even a point of discussion. Under a looser interpretation of the council mandate during meetings in early 1987, the commission also offered that it would be willing to discuss the effect (economic and ecological) of a production freeze. This move constituted "an important concession," and the negotiations were now fully engaged for the first time (Lammers 1988, 237).[39]

In abandoning its protection of absolute gains, the EC turned its attention to its relative position in export markets. The change is best represented in the attempt to develop an accounting scheme for achieving a freeze or, if agreed to, later reductions. The contrasting formulas for measuring CFC emissions proposed by the EC and the United States affected states differently depending upon their level of exports. The U.S. proposal called for an "adjusted production" approach for counting CFC emissions, while the EC vigorously defended a simple production freeze. Either scheme would allow about the same absolute levels of CFC production, but under the adjusted production approach the EC would have to halt exports in order to avoid heavier cuts.[40] At stake was the relative position of the EC as the major exporter to the international market. Under a simple production freeze the EC would not be required to give up its position as the dominant international supplier. The EC was now pursuing relative position, so there was a "vigorous rejection of the adjusted production approach by the European Community" (Lammers 1987, 251).

As framing predicts, under a losses frame the EC abandoned its protection of status quo production levels and attempted instead to maintain a relative position as the major exporter to global markets. In addition, the EC was more risk acceptant than when operating under a gains frame. In order to pursue a relative gains strategy, the EC had to gamble on the proposition of altering the content of an ozone protocol in such a way as to assure that its relative position was not compromised. The cost of entry into the negotiations was a concession on absolute production levels represented by the aerosol ban. Prior to Vienna, the EC operated under a gains frame and, because it was pursuing absolute gains, was unwilling to make that concession. After Vienna, the preference order switched, and the concern for relative position superseded the pursuit of absolute gains.

CONCLUSION

The second round of negotiations resulted in the Montreal Protocol, which secured a rough compromise between the U.S. position of a total production ban and the initial EC position of protecting the status quo. The diplomatic history of EC participation outlined here agrees with the common view that a concern for ecological stability initially propelled the dissemination and subsequent discussion of the ozone-depletion hypothesis, which forced the issue of ozone-layer protection to prominence on the international political landscape. Yet, it departs from the common view by also demonstrating that the compelling issues in actually securing a treaty were economic, not environmental. One observer puzzled: "Why, when professionals cannot make up their minds even about what is happening in the Antarctic, should the World's diplomats be locked in negotiations?" (Maddox 1987, 101). The answer that emerges from this study is that an ecological issue quickly became dominated by economic concerns. It is for this reason that a focus on gains preference captures the essence of the struggle to achieve an ozone agreement.

The theoretical contribution of prospect theory lies not only in describing the origins of the initial EC gains preference but also in its ability to track the dynamics by which this preference changed. In an important sense the Montreal case represents well the difficulty in sustaining the division between realism and liberalism. As currently developed, both positions hold that state goals are consistent across contexts. Thus, the EC's shift in gains pursuit highlights the inherent weakness of the current theoretical bifurcation. The attempt to fix a single goal for states ignores the empirical challenge of a world in which states exhibit a broader set of behaviors. The actions of the EC during the protocol negotiations are consistent with the new framework offered here. The content of the social frame accurately predicts its policy. In a gains frame the EC exhibited risk aversion and absolute gains pursuit, while in a losses frame it became risk acceptant and pursued relative advantage. Caution is warranted, however. The most that can be claimed here is that prospect theory allows for a logic of state choice which accommodates both relative and absolute gains pursuit, and this logic can explain behavior treated as anomalous by traditional theory. The general validity of the new framework awaits further study.

Nevertheless, the strength of the new framework is enhanced because a common alternative explanation can be dismissed. It might be mistakenly inferred from the Montreal case that the EC's acquiescence to a negotiated settlement resulted primarily from the American threat to exercise power. As noted previously, however, it is not clear that the United States would have acted, and the EC had fought off a previous American bilateral attempt to secure concessions. Rather, the evidence suggests that threatened U.S. action was one in a number of changes in the political topography which together solidified a losses frame for the EC. More important, even if we were to grant the traditional power explanation that the EC was forced into negotiations, *such an explanation cannot account for the shift in EC policy from absolute to relative gains pursuit*. That is, there is no logic to the application of power that would force a state to shift from absolute to relative gains, and there is no evidence that the United States desired such a shift in EC goals. The (attempted) exercise of power by the United States does not alone provide an explanation for the midstream change in EC gains preference exhibited during the Montreal negotiations.

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[39] It should be noted that this shift was not the result of compelling science, as the EC position on the validity of the scientific conclusions linking CFCs to ozone depletion had not changed. As late as March 1987, the French environmental minister denied the existence of any demonstrable link between the two.

[40] For a complete and detailed discussion which develops the mathematical logic of different schemes, see Lammers 1987.

Yet, power is not unimportant for this analysis. In the EC case the threat of U.S. power contributed to a losses frame. When combined with the notion that states in the losses frame exhibit greater risk acceptance and pursue relative gains, a clear logic emerges that connects the application of power with state actions and goals. The conventional understanding of power as a tool to circumscribe rival action is now supplemented with the notion that power can inadvertently alter the content of rival policy goals in predictable ways. Here, the U.S. threat of domestic trade legislation helped shape a losses frame for the EC and altered its gains preference. The shift was from a gains to a losses frame, and thus from absolute to relative gains pursuit. While not explored in this analysis, it is logically possible that the application of power through rewards can also change the target state's social frame from losses to gains and, therefore, its policy goals from relative to absolute gains pursuit. This new dimension to the application of power begs further examination.

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Hegel and the Ontological Critique of Liberalism

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Contemporary communitarians argue that a proper consideration of ontological questions of identity and community forecloses deontological liberalism as a viable option, since deontological liberalism cannot ground a sufficiently strong form of immediate identification with the ethical life of the community. Hegel's ethical theory constitutes perhaps the most fully realized account of such identification. Yet, I argue, Hegel's ethics does not require the strong form of immediate identification required by communitarians. My analysis emphasizes the strands in Hegel's account which suggest that a weaker form of identification, reconcilable with deontological liberalism, can plausibly ground a stable form of civil society. In particular, Hegel's notion of patriotic trust is designed to ground both a direct and a reflective relation between individuals and their ethical and political tradition. Hegel's analysis anticipates and offers insights to supplement recent accounts of trust as a form of social capital.

Is some form of identification with ethical norms and institutions a necessary condition of viable social life? If so, what form of identification is required? Contemporary communitarian theory argues for a particular form, which I will call *strong identification*. Because liberalism is unable to ground strong identification, communitarians suggest, it offers a flawed conception of ethical life. I will argue that, while some form of identification may in fact be necessary to ground viable social life, communitarian arguments requiring strong identification are unfaithful to the Hegelian insights which motivate communitarian thought.

Contemporary communitarian theory is grounded in the Hegelian insight that an adequate form of ethical life must express the identity of its members, rather than impose rationally derived criteria on ethical deliberations. Communitarian argument is not primarily oriented toward a critique of institutions at the practical level.[1] Rather, by "open[ing] up the ontological questions of identity and community" (Taylor 1989a, 165), communitarian thought delimits the set of viable ethical theories. In particular, communitarians maintain that a proper consideration of ontological issues forecloses deontological liberalism as a viable option.

In order to provide a plausible basis for a society, it is argued, an ethical theory must provide an account of the normative basis grounding the shared sacrifice and cooperation necessary to sustain social cohesion (see Sandel 1984, 94, and 1989, 537; Taylor 1989a, 165ff, and 1995, 204). In the absence of coercion, only an ethics with which individuals can identify both *convergently* and *immediately* can provide such a basis. Individuals identify convergently when they coordinate on a practice which produces a public good. They identify immediately when they identify with the process of realizing the practice itself as a joint endeavor. Immediate identification is a necessary condition of stable ethical life because mere instrumental rationality cannot ground the necessary "willing identification" with the community. Such identification requires, as its basis, "a sense that the political institutions in which they live are an expression of themselves" (Taylor 1989a, 165).[2]

Deontological liberalism, however, appears to ground only convergent identification. The central concern of deontological liberalism is a framework of principles designed to secure freedom, stability, and prosperity. Because they are abstract and instrumental, such principles cannot ground a shared sense that society embodies an expression of the self. Therefore, such an ethical theory is not fully attentive to the relation between identification and social cohesion. In its abstraction, deontological liberalism ignores an ontological prerequisite for a viable form of ethical life. Thus, communitarians argue, the theory rests upon implausible ontological assumptions.

This indictment of liberalism appears to rest on an unargued assumption regarding the nature of immediate identification. The communitarian argument assumes that immediate identification requires individuals to identify directly with the entire set of norms and practices of their community. I will call this *strong identification*. I suggest, however, that there is a less radical form in which individuals identify with a set of basic or fundamental principles that define the society's ethical character and that constrain the possible content of the less fundamental norms. I will call this *weak identification*. While strong identification is completely inconsistent with deontological liberalism, weak identification is not: The liberal identification with norms and principles of justice may be understood as a form of weak identification.

Competition between strong and weak accounts of

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[1] See, however, valuable discussions of the institutional and policy implications of the communitarian argument in Avineri and de-Shalit 1992; Etzioni 1995a and b; Glendon 1991; and Paul, Miller, and Paul 1996.

[2] This kind of identification could be absent for two different reasons: (1) We fail to identify and defend the substantive value our ethical life embodies (see Sandel 1989, 537); or (2) the substance of our ethical life has necessary logical extensions with which we do not or cannot identify (see Sandel 1984, 94). Both points, however, are intended to demonstrate the inadequacy of a framework of principle as the center of our ethical life.

social identification has remained a contentious matter in modern political thought and has played a particularly significant role in the evolution of modern legal theory. Since legal decisions determine the precise substantive content of positive law by interpreting the nature and function of that legislation as the product of a particular context and ethical tradition, the jurist requires a theory of the relation between the community's ethical norms and its positive law. If our understanding of the role of identification in ethical life influences our beliefs regarding the relation between norms and law, then this understanding may profoundly influence the evolution of our legal tradition and the content of our civic culture.

The opposition between strong and weak identification has been posed most starkly, in modern legal theory, by the Hart-Devlin debate over the legal enforcement of morals. Devlin (1965) argues from the overriding social significance of strong identification, asserting that the normativity of much positive law derives from the necessity of social identification with the moral content embodied in the statute.[3] Hart (1961, 97–107), implicitly embracing the argument for weak identification, maintains that the normativity of law derives from our recognition of fundamental rules which determine how rules of law are to be identified, not from our identification with a moral principle embodied in the statute (see Hart 1963).

While Hart's position has represented the mainstream of modern American jurisprudence, arguments of the type advanced by Devlin continue to influence legal theory and practice. In a particularly salient instance, the Supreme Court relied on an appeal to existing social norms to justify its decision limiting privacy protections to heterosexual couples.[4] Tribe (1988) claims that the argument from social norms was of minor significance in the decision and cites indications that "prejudice rather than legal principle was responsible for the outcome."[5] Communitarians, however, have embraced the form if not the substance of the majority's argument, asserting the plausibility of direct reliance on social norms and values in the justification of legal decisions.[6] These arguments have evoked a significant, if not unequivocally favorable, response in the legal literature (see Glendon 1991, Moore 1989, Sandel 1989).

Sandel (1996) makes a particularly forceful case for the essential role of background moral norms in generating and sustaining the legitimacy of law and policy. While he does not argue that legitimate rules of law must be grounded in generally recognized moral norms, he *does* argue that legal disputes involving conflict over moral issues can only be persuasively resolved by appealing to underlying shared values of the community (Sandel 1996, 100–3, 107).[7] In limiting persuasive legal justification to appeals to shared community values, Sandel endorses the reasoning, if not the result, of the *Bowers* decision. Implementation of Sandel's notions of legal reasoning would involve a major shift in legal theory and practice away from the mainstream positivism which Hart's work grounds. Such a shift might significantly reduce the power of positive law to shield ethnic and cultural minorities from coercive applications of the norms of the majority culture.

Thus, important strands of modern legal and political discourse continue to assume that strong identification plays an essential role in the most persuasive account of viable ethical life. This assumption continues to exert a potentially significant influence upon policy and legal thought. Fukuyama (1995, 5), for example, grounds his influential critique of "social engineering" (such as the Clinton administration's health care reform proposals) in his assertion that productive cooperation must ultimately be grounded in a society's cultural tradition, a tradition that can be shaped directly *only* "through an increased awareness and respect for culture."

Strong identification, I will suggest, is neither necessary for nor entirely consistent with a coherent account of identification within an ethical community. I will evaluate the relative merits of strong and weak identification as grounding elements of viable ethical life by examining Hegel's account of the role and significance of immediate identification, which has inspired much of contemporary communitarian thought. Hegel's ethical theory constitutes perhaps the most fully realized account of immediate identification with the ethical life of the community. It is grounded in the notion that ethical life must be "based on an identification with others in a particular common enterprise" (Taylor 1989a, 166). In addition, it has "inspired" much of contemporary communitarian thought (see Avineri and de-Shalit 1992, 2). Yet, I will argue, Hegel's ethics requires merely weak identification.

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[3] "A recognized morality is as necessary to society as a recognized government . . . [thus] society may use the law to preserve morality" (Devlin 1965, 11).

[4] *Bowers* v. *Hardwick*, 106 S. Ct. 2841 (1986).

[5] "The 'good' news about the Court's decision in *Bowers* v. *Hardwick* is, paradoxically, that it probably was not decided on principled grounds. . . . Indications that prejudice rather than legal principle was responsible for the outcome can be found in what the four dissenting Justices called the majority's 'almost obsessive focus on homosexual activity'" (Tribe 1988, 1430–1).

[6] See, for example, Moore's (1989, 550) claim that "nothing that is morally good or bad can be excluded in judging the morality of a political institution without skewing one's judgments about the all-things-considered desirability of that institution." In addition, see Glendon's (1991, 143) claim that "the inadequate attention to human sociality and civil society in mainstream American law and political theory has practical consequences. The lack of public discourse regarding responsibility, sociality and civil society leaves us to work out our own vision of the kind of people we are and the kind of society we want to become."

[7] "[Minimalist liberalism] proposes bracketing controversial moral and religious issues for political purposes. . . . Even given agreement to bracket an intractable moral or religious controversy for the sake of social cooperation, it may still be unclear what counts as bracketing. And resolving that question . . . requires either a substantive view about the moral and religious interests at stake or a conception of the person such as Kantian liberalism affirms. But both solutions would deny liberalism its minimalism; each would implicate its putatively political conception of justice in moral and philosophical commitments it seeks to avoid" (Sandel 1996, 100–3).

"The neutral case for toleration . . . leaves wholly unchallenged the adverse views of homosexuality itself. But unless those views can be plausibly addressed, even a court ruling in their favor is unlikely to win for homosexuals more than a thin and fragile toleration" (p. 107).

If Hegel's account of ethical life requires merely weak identification, then Hegel's ethics is in tension with contemporary communitarian theory. Ironically, this tension results precisely because communitarians *disagree* with Hegel regarding the domain and scope of ontological inquiry. The concerns of the "ontological critique of liberalism" are limited to "the issues of identity and community" (Taylor 1989a, 165). Thus, the communitarian analysis treats its central concern, the normative basis of viable ethical life, as a discrete question that can be analyzed without requiring a reconsideration of the ontological status of social life itself. While reflecting contemporary discomfort with applied metaphysics, such an approach is problematic for a theoretical position that claims to develop Hegelian insights.

Hegel (PR, 10) argues that proper comprehension of right, ethics, and the state "presupposes a familiarity with [the] scientific method" developed in his *Science of Logic*.[8] The principles of this method "are to be sought in a system of thought-types or fundamental categories" (EL, 37). True, and therefore objective, determinations according to this method must form part of a system or "totality of thought" that relates the social issues in question to the more fundamental categories.[9] Thus, for Hegel, fundamental social questions can only be examined adequately in light of even more fundamental categories of thought that determine the ontological status of social life. In such an analysis, Hegel claims, "the opposition between subjective and objective, in its usual sense, vanishes" (EL, 37). Thought which is thus informed by "scientific" method recognizes that "in the other, one meets oneself" (EL, 222). This notion of subject-object identity plays a central role in Hegel's political thought, and it leads Hegel to an understanding of immediate identification quite different from that of contemporary communitarians.

Just as "scientific" thought determinations dissolve the ordinary opposition between subjective and objective, so "scientific" political determinations must dissolve the opposition between the individual, as subject, and his or her form of ethical life, as object. Hegel's strategy for achieving this goal involves a strand of his argument of particular relevance to contemporary political discourse. I will argue that Hegel's notion of patriotic trust is designed to ground both a direct and a reflective relation between individuals and their tradition.

Hegel's analysis of trust as the necessary condition of stable social cooperation anticipates, and offers insights to supplement, recent accounts of trust as a form of social capital (see Dunn 1990, Fukuyama 1995, Gambetta 1988, Hardin 1991, Putnam 1995). Contemporary theorists disagree regarding the necessary conditions for fostering this form of social capital. Does trust result from rational calculation, habituation, disposition, or a combination of these? Hegel argues that trust is a form of disposition realizable within certain forms of ethical life. In a later section I will suggest that Hegel anticipates and supplements Dunn's (1990) account of trust as a modality of action involving both rational reflection and sentiment. Moreover, Hegel's notion of trust, involving both reflection and sentiment, offers a valuable resource for his project of resolving the modern division between the subjective desires of individual self-consciousness and the universalistic demands of rationalist ethics.

In the next section, I will examine the relation of Hegel's ontological thought to his account of immediate identification. I then will argue that (1) a Hegelian account of substantial ethical life will involve a complex equilibrium between unmediated identification and reflective evaluation; and (2) weak rather than strong identification will characterize such an equilibrium. I will argue that Hegel's discussion of trust, the political disposition, develops an account of weak identification in substantial ethical life. I also will examine some institutional implications of Hegel's account of weak identification.

HEGELIAN ONTOLOGY AND IMMEDIATE IDENTIFICATION IN ETHICAL LIFE

While ontological inquiries traditionally investigate the nature of what exists, Hegel's ontological thought investigates which "forms of thought" are capable of containing truth (EL, 41). Hegel defines the problem in this way because he believes that "thought is the constitutive substance of external things" (EL, 37).[10] In this section, I will examine this strand of Hegel's thought and its influence on his account of immediate identification in ethical life. I will argue that Hegel's ontological thought grounds both (1) his claim that objective thought dissolves the opposition between subjectivity and objectivity and (2) a complex understanding of the role of rational principle in ethical life.

Ontology and Subject-Object Identity

What is the status of Hegel's claim regarding the constitutive role of thought? Does Hegel wish to argue that objective thought literally determines "external things," or does he intend to characterize the role of thought in determining what could count as a determinate object?

According to one influential interpretation, best articulated by Taylor (1975), Hegel argues that objective thought literally determines the structure of reality through its dialectical movement.[11] While certain categorical concepts "must apply to anything which can be a reality for us," these concepts nevertheless involve a "contradiction" that tends to "annul" the concept (Taylor 1975, 228–9). Such contradictions, however, are "necessarily related to another [categorical concept] which resolves the contradiction at its level" (p. 230). The partial reality to which a contradictory concept applies "both must exist and yet destroy itself" (p. 230). Such a self-undermining reality can only continue to exist, Taylor argues, "if it is continually posited by the larger order of which it is a part" (p. 230).

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[8] The following abbreviations of Hegel's works are used throughout: EL = Hegel [1827] 1975; PG = Hegel [1807] 1979; PM = Hegel [1827] 1971; PR = Hegel [1821] 1991; and SL = Hegel [1816] 1969. H and G are standard notation referring to additions to the text of PR based on lecture notes by H. G. Hotho and K. G. von Griesheim, respectively, which have been published as part of the text since the 1933 edition.

[9] "Truth, then, is only possible as a universe or totality of thought. . . . Unless it is a system, a philosophy is not a scientific product. . . . The term *system* . . . [denotes] a genuine philosophy [that] makes it a principle to include every particular principle" (EL, 20).

[10] "The real nature of the object . . . is a *product* of *my* mind" (EL, 35). "The finitude of cognition lies in the presupposition of a world already in existence" (EL, 284).

In order to establish that dialectical movement among categorical concepts is constitutive of external things, however, Hegel would need to demonstrate that such dialectical movement, in fact, characterizes relations among specifiable categorical concepts. Hegel's argument for a contradiction in the concept of determinate being (EL, 136–41), Taylor maintains, is offered as such a demonstration. In Taylor's account, however, Hegel's argument "builds . . . on an equivocation on the concept of negation" (1975, 238; see 236). Hegel claims that the limit, or boundary, implicit in the notion of determinacy both "makes the reality of a thing" and simultaneously negates it (EL, 136). Thus, determinacy "contains [its] negation within itself" (Taylor 1975, 238). Yet, the boundary implicit in determinate being negates quantitatively, through an extension of the object's physical frontier. This *quantitative* sense of negation cannot ground a contradiction with the *qualitative* concept of determinate being. Hegel's claim for a contradiction apparently fails.

Yet, Hegel's account of negation involves an equivocation only if his analysis refers to the negation of concrete objects. Hegel, however, warns that the concepts in his analysis are "to be taken in [their] indeterminate simplicity" (SL, 83). If the negation "contained" in determinacy is itself indeterminate, it cannot plausibly be represented as the extension of a physical frontier.[12] Rather, Hegel's notion of negation appears to be qualitative, referring to the instability and possible extension of the set of criteria distinguishing a particular finite thing from other finite things.[13] Such a qualitative sense of negation *can* plausibly ground a contradiction with the qualitative concept of determinate being. It is Taylor's criticism, not Hegel's claim for a contradiction, which fails.

The failure of this criticism, however, reveals an important quality of Hegel's argument. Hegel focuses narrowly on "the laws and characteristic forms" of thought (EL, 25). It is precisely because he wishes to provide an account of the operations of *thought* that he discusses only the indeterminate "conceptual conditions required for there to be possibly determinate objects of cognition" (Pippin 1989, 176). Yet, this self-consciously restricted focus necessarily qualifies all of Hegel's ontological claims. If his primary concern is the role of thought in determining what can possibly be a determinate object,[14] then Hegel may intend to restrict his ontological inquiries to an exploration of "the relation of spontaneous human thought to objectivity" (Pippin 1989, 252).

Hegel's explanatory comments suggest that he intends his argument in this way (see EL, 37–9).[15] For example, in order to illustrate the claim that "objective thought [constitutes] the heart and soul of the world" (EL, 37), Hegel examines the constitutive status of a specific proposition: Every "particularized" animal is a member of a general class (the class of animals). He first notes that the property of "being an animal . . . constitutes [the animal's] definite essence" (EL, 37). If Hegel were offering a literal claim for the constitutive role of thought, then we would expect him to argue that our determinations regarding this property make possible the existence of its instances (animals in the external world). Instead, Hegel offers a claim regarding the role of this property in our *determinations* relating to animals: "Take away from the dog its animality, and it becomes *impossible to say what it is*" (EL, 37, emphasis added). Hegel argues that this property is constitutive of our determinations, not of the object itself. Thus, Hegel's ontological investigations develop a complex claim regarding the kind of thought (objective thought) that can make a finite object determinate.

If the determinateness of external things merely constitutes the embodiment of objective thought determinations, then any determinate object constitutes an extension of some subject. Hegel therefore has defended his claim that objective thought dissolves "the [ordinary] opposition between subjective and objective" (EL, 37). His account of ethical life locates this insight centrally, employing it in an attempt to resolve the modern division between the subjective desires of individual self-consciousness and the universalistic demands of rationalist ethics. Hegel believes that if the ethical life of a community incorporates such a dualistic division between the demands of morality and the desires of the individual, members of that community will necessarily be alienated from the ethical fabric of their society (Hyppolyte [1946] 1974, 483f; Taylor 1979; Wood 1990, 90f, 200f).

If such dualism is to be avoided, then ethical life must express rather than constrain the identity of the individuals who are the subject of ethics (Hyppolyte [1946] 1974, 3–27; Williams 1992, 121–41; Wood 1990, 213f). If objective thought can dissolve the dualism that alienates the individual, as subject, from her form of ethical life, as object, then it will allow individuals to "discover their essential self-consciousness in [social] institutions" (PR, sec. 264, 287). Such identification with social institutions will not follow simply and directly from objective thought. Rather, objective, or "scientific," thought makes possible the construction of a political theory designed to realize the desired form of identification. It is for this reason that Hegel describes the *Philosophy of Right*, his most ambitious work of political theory, as presupposing "the philosophical manner of . . . conducting a scientific proof" as "the method which constitutes its guiding principle" (PR, 10).

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[11] "In discovering the necessary relations between the categorical concepts of a transcendental logic, we will also be discovering the necessary structure of reality" (Taylor 1975, 227).

[12] Hegel states explicitly: "The view of a limit [and the negation it implies] as merely an external characteristic . . . arises from a confusion of quantitative and qualitative limits" (EL, 136).

[13] Such instability would lead to negation if the extension of the qualitative bounds of one finite thing were to undermine the qualitative boundaries of other finite things.

[14] See Pippin (1989, 251): "Hegel assumes that . . . the requirements of a self-conscious judger of objects . . . do determine what could be a determinate object."

[15] Of course, some of Hegel's claims in the *Logic* imply a more ambitious ontological project that fits Taylor's account more closely. As Pippin (1989, 188) argues persuasively, however, "even in some of the most apparently extravagant such passages, it is still possible to continue to make out" an argument limited to claims regarding "the relation of spontaneous human thought to objectivity" (p. 252).

While forms of alienation will necessarily persist within Hegel's account of ethical life, they will differ qualitatively from the alienation produced by formal rationalist systems of ethics. In particular, alienation in ethical life will be inherently unstable, since individuals *will* identify with their norms and institutions once they understand that their ethical tradition embodies their essential self-consciousness (see Hardimon 1994, 93ff). While individuals may fail to identify immediately with positive expressions of ethics in law, institutions, and norms, they will positively affirm these elements of ethical life once their role in the satisfaction of fundamental needs and desires is understood.

Rational Principles and Ethical Life

If social institutions embody the essential self-consciousness of individuals, then *should* people identify directly and unreservedly with those institutions? That is, does Hegel's argument establish that strong identification is at least the appropriate orientation toward one's ethical tradition, if not a necessary condition for viable ethical life? Hegel appears to make precisely this argument in the *Phenomenology of Spirit*. The valid law of the community, he argues, is "the absolute *pure will of all* which has been given the form of immediate being" (PG, 260). In affirming such laws, "ethical *self*-consciousness is *immediately* one with essential being" (PG, 261). The testing of such laws against criteria of reason constitutes "the insolence of a knowledge which argues itself into a freedom from absolute laws, treating them as an alien caprice" (PG, 260).

Yet, these passages are deceptive. In fact, Hegel believes that a community's ethical tradition must be rational in order to satisfy the needs and desires of its members (PR, 21[16]; sec. 4, 35; see Forster n.d.; Westphal 1993; Wood 1990). Moreover, since positive law does not always embody "that right which has being in and for itself," it is "imperative" for each individual to evaluate the rationality of the elements of her ethical tradition.[17] But can Hegel argue consistently that the ethical individual not only must "[stick] steadfastly to what is right [because it is what is right,] . . . abstaining from all attempts . . . to shake or derive it" (PG, 262) but also must evaluate the rationality of positive law?

Hegel can maintain these claims consistently because of the complexity of his understanding of the relation of objectivity, right, and reason. He argues that "objective," or true,[18] thought determinations must be grounded in an appreciation of the systematic interrelation of the infinity of objects constituting "reality."[19] Principles and laws are rightful and, therefore, rational in terms of philosophical science if they are grounded in objective determinations.[20] The objective thought of members of the community constitutes the rational, or rightful, laws and institutions necessary to create what self-consciousness desires: rational freedom (PR, 10; sec. 4, 35).

If the community's objective thought has constituted an ethical and legal tradition, however, then Hegel rejects the authority of abstract reason to undermine the validity of that tradition. Thus, Hegel rejects Kant's notion of an abstract rational principle or "universal fixed rule" (PG, 258)—such as the law of noncontradiction—as an appropriate criterion for evaluating the justice of legislation. Yet, Hegel does not argue that individuals should simply identify with the positive contents of their legal tradition. In fact, as we have seen, he *requires* that the individual assess the rationality of his tradition. Hegel simply argues that such an assessment should not test the tradition against an abstract principle or set of principles.

Moreover, Hegel only offers a qualified rejection of the role of principle in constituting an ethical tradition. While he argues that abstract rational principles are an inadequate criterion for assessing the justice of legislation, he does not argue explicitly that the contents of a just ethical tradition should not take the form of principles. Hegel argues that the contents of an ethical tradition must satisfy two conditions: such contents must embody objective thought and further the realization of rational freedom. If these criteria are satisfied by an ethical tradition whose content takes the form of rational principles, then an ethics of principles may be consistent with Hegel's goals of actualizing freedom and eliminating dualism. While Hegel rejects rational principles as criteria of the validity of a form of ethical life, the role of such principles as elements of the substance of an ethical tradition is left indeterminate in Hegel's ontological thought.

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[16] "This treatise . . . shall be nothing other than an attempt to *comprehend and portray the state as an inherently rational entity*" (PR, 21).

[17] "It is imperative for us to learn to recognize precisely what right is" (PR, 13). "The human being must . . . consider the rationality of right" (PR, 14).

[18] "The term "Objective Thoughts" indicates the *truth*—the truth which is to be the absolute *object* of philosophy" (EL, 45).

[19] "Truth . . . is only possible as a universe or totality of thought" (EL, 20).

[20] The analysis of the *Philosophy of Right* presupposes, "the philosophical manner of . . . conducting a scientific proof as the method which constitutes its guiding principle" (PR, 10).

Summary

The insight that a community's institutions embody the essential self-consciousness of its members grounds both Hegel's claim that immediate identification can mediate the dualism dividing the individual from her form of social life and Hegel's rejection of abstract reason as a privileged criterion for evaluating the contents of an ethical tradition.

Yet, the status of reason and reflection in Hegel's argument remains ambiguous. His assertion that individuals "discover their essential self-consciousness" in institutions suggests a direct relation, unmediated by reflection. The "objective thought" grounding such a "discovery" is, however, intrinsically reflective, involving an examination of fundamental social questions in light of even more fundamental categories of thought. Moreover, Hegel claims that his account of ethical life will constitute "nothing other than an attempt *to comprehend and portray the state as an inherently rational entity*" (PR, 21).

In order to translate his ontological conclusions into a substantive account of ethical life, Hegel must (1) provide an account of the nature of social institutions capable of embodying the self-consciousness of members of the community and (2) define the status and role of reason in such an account.

REFLECTIVE IDENTIFICATION AND PATRIOTIC TRUST

Hegel's goal of resolving dualistic divisions in ethical life leads him to argue against abstract reason as a criterion for evaluating the contents of an ethical tradition. Yet, he nevertheless appears to accept the Kantian premise that free action must be grounded in reflective choice: For Hegel, "to be free is to be self-determined. Indeed, it is to be self-consciously self-determined" (Schacht 1972, 324). If substantial ethical life is to realize human freedom, then any account of substantial ethical life grounded in unreflective choice must be problematic. Thus, at a fundamental level, strong identification seems inconsistent with Hegel's goal of realizing human freedom in ethical life.

Hegelian theory therefore incorporates a tension between its two motivating insights: Free choice must be reflective; and immediate identification requires a direct and unmediated relation between the desires of the individual and the substance of ethical life. The first premise appears to require, and the second bar, the application of some reflective evaluative criterion in ethical deliberation.

In this section, I will examine the tension between the strands of Hegel's analysis which require and which bar reflective evaluative criteria in ethical deliberation. I will argue that Hegel's account of patriotic trust is designed to address this tension. I will suggest that his attempt to resolve this tension leads him to an understanding of immediate identification that requires merely weak identification.

Reflection and Ethical Deliberation

Hegel maintains that philosophy must recover the sense of "solid and substantial being" that the modern spirit has lost (PG, 4). In order to achieve "substantial being," individuals must identify with the ethical judgments of their community, as embodied in laws and customs. If they do so, Hegel argues, then the community's ethical concepts can refer to an actuality in the world: the ethical tradition of that community.

Hegel's strong emphasis on direct identification has led some commentators to conclude that his account of ethical life requires strong identification. Parkinson (1985, 16a), for example, notes the charge of "sociological determinism" brought against Hegel by Popper, among others. While Parkinson attempts to defend Hegel from this charge, certain communitarians have embraced Popper's position, asserting that the only viable form of ethical life requires strong identification. Sandel (1996, 53), for example, cites approvingly Frankfurter's assertion that "the ultimate foundation of a free society is the binding tie of cohesive sentiment."

It is important to note that, while strong identification may appear to offer a plausible basis for the immediate identification necessary to ground stable ethical life, Hegel never argues for strong identification as the necessary condition of "willing identification" with the community. Moreover, he asserts that if the state is to realize substantial ethical life, then it must preserve the *particular* standpoint.[21] Since the particular standpoint includes the capacity for independent evaluative judgment, preservation of this standpoint seems at least prima facie inconsistent with strong identification. Thus, strong identification appears to constitute *at most* a sufficient rather than a necessary condition for stable ethical life.

More significantly, however, strong identification appears to be tangibly inconsistent with essential features of Hegel's account of substantial ethical life. This claim can be defended most readily by examining the forms in which strong identification may manifest itself in ethical life. Strong identification might be the product of two distinct mechanisms: (1) individuals might unreflectively and automatically identify with the jointly constituted ethical tradition of their community, or (2) individuals might reflectively identify with that tradition *because* it is the community's jointly constituted tradition.

It seems unproblematic that Hegel must reject the first mechanism, since reflection is essential to his account of substantial ethical life. He criticizes the ethical life of early Greece precisely because it was not reflective and thus not self-consciously willed (PG, 214, 280). Although individuals identified directly with the ethical determinations and norms of the community,[22] this identification was merely "immediate." The ethical order existed as something "given" rather than as ethical substance willed self-consciously (PG, 214, 280). The legitimacy of Greek ethical life was progressively undermined as awareness developed that "given" ethical concepts belonged to the medium of thought, and therefore no guarantee existed that such concepts corresponded to any concrete actuality in the empirical world. In order to avoid the element of immediacy that flawed Greek ethical life, the ethical order must be self-consciously willed.

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[21] "Individuals, as citizens of the state, are private persons who have their own interest as their end" (PR, sec. 187, 224).

[22] Reason both "realized its essential character" and "lived in it" (PG, 214).

Moreover, Hegel argues explicitly that the nature of right is far from being automatically and self-evidently present to consciousness: "It is . . . difficult to recognize the [political] good and its inner necessity" (PR, sec. 268, 289).[23] Rather, the nature of right must be self-consciously sought.[24] The end of reason in the state is "to eliminate the *immediacy* and *individuality* in which spirit is immersed, so that this externality may take on the rationality of *which it is capable*" (PR, sec. 187, 225). Thus, political theory is necessary "to *discover* the necessity at work here" (PR, sec. 189H,G 228, emphasis added). A Hegelian account of strong identification would necessarily take the form of *reflective* and self-conscious identification.

Reflective strong identification could itself have two sources: (1) the process of joint constitution may eliminate particular differences, resulting in a consensus with which all can identify; or (2) the process of jointly constituting an ethical tradition may induce solidarity and thus joint identification with the end-product, despite enduring particular differences.

Hegel also must reject the first variant of reflective strong identification. A central goal in his account of ethics is preservation of the subjective standpoint, with its own dissonant desires: "Only when both universal and subjective are present can the state be regarded as fully articulated" (PR, sec. 260, 283).[25] Hegel concedes that "our inner voice can . . . come into collision" with the contents of our ethical tradition (PR, 13). Moreover, he asserts that the possibility of such conflict is intrinsic to the nature of right in the state, rather than characteristic of any particular instantiation.[26]

In fact, such a collision seems inevitable, since (1) "the human being . . . claims to have within himself the measure of what is right" (PR, 13); (2) "distinct variet[ies] of right" exist and "are all in equal measure rights" (PR, sec. 30, 59); and (3) the variety of right realized in the state is "not absolute in an unlimited sense" (PR, sec. 30, 59). If distinct varieties of right exist and are equally valid, and if the particular variety of right realized by the state is not absolute, then the form of right recognized by some members of society will almost inevitably collide with the form of right realized by the state.

The second variant of strong reflective identification, holding that joint constitution of an ethical consensus will induce solidarity, may appear to be consistent with certain strands of Hegel's argument. For example, Hegel maintains that, while "most people regard payment of taxes . . . as an infringement of their particularity" (PR, sec. 184, 221), "taxes become higher as a result of the people's own consciousness" in constitutional states (PR, sec. 302H, 343). Thus, Hegel appears to suggest that a people jointly experiences a more universal perspective as it jointly constitutes more fully realized instantiations of the idea of the state.

Yet, such a picture is inconsistent with Hegel's most explicit account of the disposition of the individual within the state, a disposition which "holds [the state] together" (PR, sec. 268H, 289). The "political disposition" is grounded not in induced solidarity but in *trust*: "The political disposition, i.e., patriotism . . . is in general one of trust (*das Zutrauen*)" (PR, sec. 268, 288).

Patriotic Trust

In specifying trust as the content of the political disposition, Hegel addresses the tension between those elements of his political theory which respectively require and appear to bar reflective criteria in ethical deliberation. Trust can be understood both as a sentiment and as the product of reflection (see Dunn 1990, 26–7). Thus, trust grounds both a direct and a reflective relation between the individual and her form of social life. Since trust both reflects the particular sentiment of the individual and mediates that sentiment through reflection, a political disposition of trust approximates the "unity of form and content" sought by Hegelian political theory in order to reconcile the individual to the rationality embodied in the actual (PR, 22).[27]

This account of patriotism should undermine lingering suspicions that Hegelian political theory was the work of an "authoritarian thinker" (Hook 1970, 57), designed to please "the reactionary and conservative rulers of Prussia in his day" (Knox 1970, 13). Rather than argue for unreflective patriotic allegiance, Hegel anticipates the contemporary insight that viable social life relies on some form of cooperative political orientation which transcends coarse-grained economic rationality (see Elster 1979, 146; Gambetta 1988; Gauthier 1988, 151; Hardin 1991).

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[23] "It is the self-awareness of individuals which constitutes the actuality of the state" (PR, sec. 265G, 287).

[24] "The human being must encounter his own reason; he must therefore consider the rationality of right" (PR, 14n).

[25] "Particular interests should certainly not be set aside, let alone suppressed; on the contrary, they should be harmonized with the universal, so that both they themselves and the universal are preserved" (PR, sec. 261, 285). Note Kelly's (1978, 20) claim that, "on Hegel's account, . . . the effective access to higher truth depends not only on the intersection of private and public will in the modern state, but also the mediation between the 'subjective consciousness' and 'objective truth'."

[26] "In nature, the highest truth is that a law *exists at all*; in laws of right, however, the thing is not valid because it exists; on the contrary, everyone demands that it should match his own criterion. Thus, a conflict may arise between what is and what ought to be" (PR, 13). Since the right realized by the state is never absolute, such a form of right instantiates what "is" rather than what "ought to be."

[27] "To recognize reason as the rose in the cross of the present . . . this rational insight is the *reconciliation* with actuality which philosophy grants to those who have received the inner call to *comprehend*. . . . This is also what constitutes the more concrete sense of . . . the *unity of form and content*. For *form* in its most concrete significance is reason as conceptual cognition, and content is reason as the substantial essence of both ethical and natural actuality" (PR, 22).

This insight has informed much recent work examining the foundations of productive civic association (see Fukuyama 1995; Putnam 1995, 1996).[28] An important project in recent studies of social cooperation has been specification of the precise nature of trust. Is it an "encapsulated interest" (Hardin 1991), a combination of sentiment and reflection (Dunn 1990, 26), or merely the product of convention (Fukuyama 1995, 26)?[29] If Hegel's account provides insight into trust as a political disposition, uniting the subjective and the rational in the service of the social, such an account may enrich contemporary discourse regarding the bases of social cooperation.

In order to offer more than a promissory note, however, Hegel must provide an account of the nature of such a political disposition. In trusting their political institutions, Hegel claims, "private persons who have their own interest as their end" (PR, sec. 187, 224) recognize that they will most fully realize their particular desires within their community's political institutions.[30] This recognition may, however, take several forms. Private persons may perceive the state as realizing their interests because: (1) the interests of the state are identical to theirs; (2) the two sets of interests are fully consistent and mutually reinforcing; or (3) the two sets of interests are distinct, but the individual's interests are best realized when the state's interests are fully realized. The first and second alternatives, however, are ruled out by Hegel's concession that conflict over the specific nature of right is intrinsic to the nature of right in the state. Trust, therefore, involves the private person's recognition that, while her ends are distinct from those of the state, her ends are best realized, and perhaps only possible, within the state.

Thus, trust is neither a spontaneous phenomenon nor a form of induced solidarity. Trust does not result simply because people jointly participate in the process of constituting an ethical consensus. Most significantly, individuals reserve the right to challenge the content of their ethical tradition from the standpoint of right: "The human being . . . claims to have within himself the measure of what is right" (PR, 13). Hegel's account of trust, therefore, seems inconsistent with strong identification. Individual members of a substantial ethical community do not identify spontaneously with their ethical tradition nor do they necessarily identify with the entire content of that tradition.

Nevertheless, this strand of Hegel's argument does not necessarily point to weak identification as essential to stable ethical life. Weak identification involves self-conscious identification with the content of a particular ethical tradition because that content embodies the substantial implications of more general norms or principles. Trust, however, is a disposition rather than a fully self-conscious attitude: "People's apparent political disposition should be distinguished from what they genuinely will. . . . They trust that the state will continue to exist and that particular interests can be fulfilled within it alone; but habit blinds us to the basis of our existence" (PR, sec. 268H, 289).

Trust, the "genuine" content of the individual's will, is apparently grounded in determinations which are not fully self-conscious regarding the relation of the ends of the individual to those of the community. The precise role of self-conscious reflection in Hegel's account of trust will determine whether the notion is consistent with weak identification.

To what degree is Hegelian trust reflective? Hegel's account of trust among individuals suggests a partial answer. I trust a person, Hegel asserts, "if I believe that he has sufficient insight to treat my cause as if it were his own, and to deal with it in light of his own best knowledge and conscience" (PR, sec. 309G, 348). The person, or institution, I trust may not share my ends or even pursue a set of ends substantially similar to mine. But its actions are best designed to realize my end "as though it were [its] own." Thus, the "secret of the patriotism of the citizens" is the recognition that "it is the state which supports their particular spheres and the legal recognition, authority and welfare of these" (PR, sec. 289, 329–30).

The trusted individual in Hegel's example is, in fact, acting as representative in furthering the ends of another individual. Similarly, the state, in sustaining the context in which the individual can further her ends, is acting as her representative. Pitkin (1967, 155) argues persuasively that "the substance of the activity of representing seems to consist in promoting the interest of the represented . . . in such a way that he does not object to what is done in his name." While the representative need not consult the literal wishes of the represented before acting, the wishes of the latter "must be potentially there and potentially relevant" (p. 155). Similarly, the individual trusts that the institutions of the state will further her particular ends without requiring that the state further her literal wishes.

The agent's trust is not grounded in a fully reflective evaluation of the role of social and political institutions in advancing the ends of the individual. The individual's "consciousness that my substantial and particular interest is preserved and contained in the interest of [the state] . . . has become second nature" (PR, sec. 268, 288–9). Trust is grounded, rather, in the justified belief that a positive relation between institutions and the ends of its members *can* in fact be specified, that it is the state which, in fact, "supports their particular spheres" (PR, sec. 289, 330).

This notion of unarticulated but potentially specifiable justification in terms of a set of social practices and relations resembles Dworkin's (1977) account of justification within an autonomous institution. A referee for an autonomous institution (such as chess) must be able to demonstrate that her judgments constitute the best interpretation of the institution (Dworkin 1977, 101–5, 115–8). Similarly, a community's norms and institutions must be justifiable as embodying the best interpretation of the project of realizing the ends of each member of the community. We identify with the contents of a political tradition not because we are presented with an explicit justification of each particular element but because we believe such a justification could readily be produced.

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[28] See, for example, Putnam's definition of "social capital" as "features of social life—networks, norms and *trust*—that enable participants to act together more effectively to pursue shared objectives" (1996, 43, emphasis added).

[29] "Trust is the expectation that arises within a community of regular, honest, and cooperative behavior, based on commonly shared norms, on the part of other members of the community" (Fukuyama 1995, 26).

[30] Trust "is that disposition which, in the normal conditions and circumstances of life, habitually knows that the community is the substantial basis and end" (PR, sec. 268, 289). Trust is grounded in the recognition that "particular interests can be fulfilled within the state alone" (PR, sec. 268H, 289).

It is in order to recognize the rational relation of institutions to individual ends, Hegel argues, that we need to learn "precisely what right is" (PR, 13). Only in light of such a rational relation, or trust in its possibility, can we achieve "reconciliation with reality" (PR, 22). Hegel complicates this picture by distinguishing between what the individual wills "genuinely" and what he wills "apparently" (PR, sec. 268H, 289). But Hegel is simply distinguishing the contingent and the fundamental aspects of political willing. In any contingent case, the individual may will to pay less in taxes; but in willing the fundamental nature of political society, the individual wills to pay taxes sufficient to sustain a form of ethical life with whose fundamental norms he identifies.

Since individuals trust that the positive elements of their institutions can be justified in light of a more general set of norms, patriotic trust constitutes an instance of weak identification. Thus, Hegel's attempt to specify an account of trust consistent with his criteria for immediate identification (reflective, preserving the subjective standpoint, not achieved through induced solidarity) produces an account of weak, rather than strong, identification.

Moreover, since an ethical tradition must consist of positive norms and principles,[31] while patriotic individuals identify only with a more fundamental set of norms, substantial ethical life is achieved only by *imposing* "finite" elements of an ethical tradition upon members of the community.[32] Such an imposition of positive norms may seem inconsistent with Hegel's goal of minimizing alienation by allowing individuals to discover "their essential self-consciousness in [social] institutions" (PR, sec. 264, 287). The apparent inconsistency dissolves once it is recognized that weak identification will destabilize, rather than eliminate, alienation:[33] Individuals accept positive norms once they recognize that those norms are grounded in more fundamental norms with which they identify. Thus, Hegel can consistently argue that representation merely of the general or universal interests of individuals in an ethical tradition instantiates immediate identification.

In fact, Hegel makes this claim explicitly in incorporating a variant of weak identification in his account of ethical life. In his discussion of the estates, he acknowledges that "it is not essential" for the individual's particular interests to be represented in the estates: "All that matters is that his interests should be upheld in an assembly which deals with universal issues" (PR, sec. 309G, 348). An ethical tradition that respects the fundamental interests of members of the community, without special regard for the finite determinacies of their particular existences, is consistent with Hegel's concern to preserve the particular standpoint and mediate alienation.[34]

Hegel's account of ethical life is therefore consistent with, and seems to require, a weak version of immediate identification. Yet, the realization of substantial ethical life could plausibly require that the ethical tradition remain immediately responsive to the will of members of the community; an ethical tradition grounded in durable principles would then be inconsistent with the realization of substantial ethical life. Hegel, however, addresses the question of temporal stability in his claim that, in order to ground a well-constituted ethical order, the community's collective ethical judgments must be, potentially, "eternal" or "everlasting" (PG, 260–1). Ethical substance is to take the form of an "unchangeable essence" (PG, 213; see Forster n.d.). While Hegel does not argue explicitly for this endurance requirement, it seems reasonable to infer that this requirement is grounded in his concern that the substance of an ethical order should be "scientific" or "rational." Substantial being must be grounded in "the cold march of necessity," not in "the ferment of enthusiasm" (PG, 5). If the determinations underlying an ethical order have the capacity to persist over time, then they are more likely to be consistent with the long-term interests of the society.

CONCLUSION: TRUST, INSTITUTIONS, AND ETHICAL LIFE

If the argument presented here is correct, then Hegel's account of immediate identification does not require strong identification. Rather, immediate identification takes the form of trust in fundamental institutions and norms. Trust, to Hegel, is thus both a form of social capital and a necessary condition for viable ethical life. Yet, trust may not flourish under all forms of social organization. What institutional and social arrangements are necessary to preserve and enhance this social resource? In particular, does Hegel's account of trust offer insights to supplement contemporary approaches to the preservation and enhancement of this form of social capital?

One strand of contemporary theory suggests that trust is necessarily the result of habituation (see Fukuyama 1995, 26).[35] Habituation, however, is inconsistent with the preservation of the individual's independent evaluative capacity. Hegel argues that this capacity must be preserved in order to ensure that members of the community self-consciously will, and thus sustain, the community's ethical consensus.[36] Moreover, as Hegel emphasizes, the nature of political right is not self-evident but must be self-consciously sought by individuals capable of evaluative assessment.

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[31] "Right is in general *positive*" (PR, S3, 28).

[32] "Spirit attains its actuality only . . . by imposing . . . limitation and finitude upon itself . . . and, *in the very process of adapting itself to these* limitations, by overcoming them and obtaining its *objective* existence within them" (PR, sec. 187, 224).

[33] See my earlier discussion of this point.

[34] "The ethical health of nations is preserved in their indifference towards the permanence of finite determinacies" (PR, sec. 324, 361).

[35] This kind of social capital "cannot be acquired . . . through a rational investment decision. . . . Acquisition of social capital . . . re-

A second influential strand of contemporary theory suggests that trust results from the individual's rational and self-interested calculation "based on empirically grounded expectations of another person's (or institution's) behavior" (Hardin 1991, 188–9). Thus, social arrangements encourage trust formation by structuring incentives. Yet, an approach that structures incentives in order to achieve outcomes required by abstract rational criteria seems conducive to precisely the dualistic division between abstract principles and subjective preferences which Hegel seeks to remedy.

Finally, a third strand of contemporary theory diagnoses trust as both a "passion" and a modality of human action chosen "more or less consciously" (Dunn 1990, 26). Thus, as in Hegel's account, trust can be understood both as a sentiment and as the product of reflection. Moreover, like Hegel, this account suggests that trust serves to mitigate the uncertainty associated with the "political division of labor" (Dunn 1990, 42).[37] The necessary social preconditions of trust, in this account, include freedom of speech and association, the right to choose political representatives, and the activity of political parties in constructing, reproducing, and repairing "structures of well-founded mutual trust" (Dunn 1990, 42). Ethical life, in this account, requires a "medium of identification" (Dunn 1990, 43) to integrate the standpoints of the individual and the state.

Hegel suggests the general outline of a social framework designed to ensure that social institutions can perform such an integrative function. Hegelian trust involves the belief that the trusted person (or institution) "has sufficient insight to treat my cause as if it were his own" (PR, sec. 309G, 348). If individuals are to trust their institutions in this fashion, then some mediation is necessary to integrate the state's universalistic standpoint and "the people and their division into different spheres" (PR, sec. 302, 342).[38]

The proper mediation requires that "the state [enter] into the subjective consciousness of the people, and that the people [begin] to participate in the state" (PR, sec. 301H, 342). The mediation is thus designed to ensure that individuals understand the state's role in furthering their rational interests and that the state remains sensitive to the appropriately formulated needs of its members. While Hegel does not provide an explicit account of the mediation, two passages suggest the possible content of such an account. The first describes the activity of an individual actualizing his own self-consciousness. In his own particular activity, the individual satisfies the needs of the society: "The individual in his *individual* work already *unconsciously* performs a *universal* work" (PG, 213). The second passage describes the ethical content of the family relationship: The end motivating the activity of the individual member must be the interest of the whole, while the end motivating the action of the whole must be the individual (PG, 269).

These two passages suggest the nature of the mediation that integrates the standpoints of the individual and the state: The individual agent furthers the ends of the whole in pursuing her particular ends. Hegel states this point more explicitly in *Philosophy of Right*: The actuality of the state "consists in the fact that the interest of the whole realizes itself through the particular ends" (PR, sec. 270H, 302). This requires, however, that the agent be provided with means for the pursuit of ends. Such conditions are likely to include the guarantee of property rights, protection from external coercion, and the provision of some degree of education. A society which fails to guarantee a minimal level of rights and resources may contain many members who cannot pursue individual ends and thus cannot further the ends of the whole.

This account of necessary conditions is minimal, but it does indicate a tendency: An ethical society is likely to protect rights, hinder unjustified coercion, and foster the ability to pursue ends. In order to ground a viable form of ethical life, it appears that an ethical tradition must contain certain specifiable kinds of durable norms. Thus, an ethical tradition grounded in temporally durable and universally applicable norms (i.e., principles) should be at least potentially consistent with the realization of substantial ethical life.

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quires habituation to the norms of a community" (Fukuyama 1995, 26–7).

[36] See discussion on pages 810–1.

[37] Hegel and Dunn both argue that trust mediates between members of a community and institutions whose assigned political task is consistent with the realization of the members' rational interests.

[38] Unless dispersed individuals are "mobilized," Hegel argues, they will fail to recognize and pursue policies that actualize their freedom (PR, sec. 290, 331).

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Schumpeter, the New Deal, and Democracy

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Joseph Schumpeter is known to American political scientists as the originator of an elite conception of democracy as a political "method," a conception found in his Capitalism, Socialism and Democracy *(1942). But I show in this paper that in Schumpeter's study of the development of liberal capitalist societies, he also treated democracy as a socially transformative historical tendency, one of several that he thought were propelling such societies toward a form of "democratic" socialism. Schumpeter regarded the politics of labor and the reorientation of state policy in the New Deal era as evidence of these tendencies—especially of a tendency toward the democratic reconstruction of workplace hierarchy, which he deplored. In his later work, Schumpeter sketched the outlines of a "democratic" socialist society in which the most harmful of these tendencies, in his estimation, would be curbed.*

Both his intellectual heirs and critics recognize the "extraordinary impact" of Joseph Schumpeter on Anglo-American political scientists and theorists during the last half century (Held 1987, 164). There is wide agreement that his treatment of democracy as a form of elite competition influenced some of the foundational postwar texts in political science, helping to originate a school of democratic theory that is variously labeled "contemporary," "equilibrium," "elite," or "empirical" (Ashcraft 1995, 1; Bachrach 1967, 18–22; Held 1987, 164; Macpherson 1977, 77; Pateman 1970, 3–5; Shapiro 1994, 125; Skinner 1973, 287; Swedberg 1991b, 278 n141). Anthony Downs (1957, 29 n11) wrote that Schumpeter's "profound analysis of democracy" provided the "inspiration and foundation" for his "whole thesis," adding that his debt and gratitude to Schumpeter were great indeed. And Downs was not alone in acknowledging a debt. One structuring premise of Lipset's influential article, "Some Social Requisites of Democracy," and of his book, *Political Man*, was a "definition" of democracy Lipset said he "abstracted" from the work of Schumpeter and Max Weber (Lipset 1959, 71; 1960, 45). Decades later, prominent political scientists continued to "follow in the tradition of Joseph A. Schumpeter" by borrowing a "definition" of democracy from his work (Huntington 1984, 195). Schumpeter's elite conception has even justifiably been cast as the "prototype" for works on democracy whose authors credited him less directly (Berelson et al. 1954, 305; Dahl 1956, 131 n12; Held 1987, 164; Macpherson 1977, 78 n1; Pateman 1970, 8; Skinner 1973, 287).

Those who have been influenced by Schumpeter's democratic thought or who have simply written about it have referred almost exclusively to the elite model of democracy delineated in his 1942 classic, *Capitalism, Socialism and Democracy*, that is, to his treatment of democracy as a "method" or "institutional arrangement" for arriving at political decisions, his assertion that democracy means "competitive leadership" and "the rule of the politician" rather than mass participation and popular rule, and his attempt to decouple the discussion of democracy from any set of ends or values, especially those associated with what he called "the classical doctrine of democracy" (Schumpeter [1942] 1976, 250–64, 269, 270, 282, 284, 285).[1] Not even the scholars who have produced the most recent full-length studies of Schumpeter and his work have found much more to his democratic thought than this deradicalized, elite conception (Allen 1991, Bottomore 1992, Oakley 1990, Stolper 1994, Swedberg 1991b).[2]

It is not difficult to grasp why certain features of this conception of democracy had great influence in the 1950s and 1960s, formative times in the discipline, when consigning political theory to the musty past and pursuing a "purely empirical and value-neutral political science" were conventional aspirations (Skinner 1973, 287). In the passages of *Capitalism, Socialism and Democracy* singled out for attention, Schumpeter denigrated the "classical" political theory of democracy, provided an easily operationalized definition of the democratic method, and offered an elite conception of democracy that seemed to many later commentators to

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[1] I have generally made use of Harper Torchbook's 1976 printing of the third edition (1950) of *Capitalism, Socialism and Democracy*, which is now the most readily available version of the book in the United States. Three editions were prepared during Schumpeter's life: the original (1942), second (1947), and third (1950) editions. Subsequent releases of the book—including the 1976 Harper Torchbook, the Harper Colophon reprints of 1962 and 1975, and the so-called 4th (1961) and 5th (1976) editions by Allen & Unwin—have been essentially printings of the third edition (Stevenson 1985, 5). Schumpeter indicated that he made no changes in the second (1947) and third (1950) editions, other than to add prefaces and, in the second edition, a new chapter on "The Consequences of the Second World War" ([1942] 1947, ix; [1942] 1976, 409). For all these reasons, I feel justified in putting the bracketed 1942 in the citations, to aid readers in following the chronological development of Schumpeter's thought.

[2] Some, however, have provided evidence that can support a more complex interpretation. In his astute analysis of Schumpeter's economic sociology, Bottomore acknowledges competing tendencies within Schumpeter's writings on democracy but does not treat this as the basis for a new interpretation of his work (Bottomore 1992, 37–8, 101–11).

be consistent with some of the bleaker findings of early voting studies—low levels of political knowledge, interest, and participation (Schumpeter [1942] 1976, 235–302). It is no more difficult to understand, however, why any attempt to redefine democracy as Schumpeter suggested would soon be resisted—why the meaning of "democracy," that is, should have been vigorously contested. Before long, critics pointed out the inherent limits and conservative ideological implications of the "elite" or "contemporary" school of democratic thought that fell heir to Schumpeter (Bachrach 1967, Held 1987, Lukes 1977, Macpherson 1977, Skinner 1973). Meeting the school on what many regarded as its home terrain, Pateman (1970) demonstrated that low levels of knowledge, interest, and participation among voters plausibly could be explained as a product of the undemocratic organization of certain formative institutions, and thus they could be incorporated into a critical, participatory theory of democracy.

While battles raged over these and related issues, no scholar interested in the question seriously doubted that the elite conception of democracy outlined above was Schumpeter's definitive statement on the subject. In this essay, however, I challenge this accepted view and argue that recovering the actual complexity of his political thought is an important task. From the late 1910s onward, Schumpeter theorized about democracy in two distinguishable ways. One is well known: the ways that elite groups and parties may be able to preside over a formally democratic institutional arrangement, providing some measure of political competition, but certainly not fulfilling the values of equality, participation, or human development. That elite conception of democracy as a political method appears in *Capitalism, Socialism and Democracy* and was the last and fullest expression of this dimension of his thought on democracy. But over the course of thirty years, and in a variety of works including *Capitalism, Socialism and Democracy*, Schumpeter also developed a different concept: democracy as a historical tendency implicated in social transformation.

Although both notions were related to Schumpeter's broad theory of the development from liberal capitalism to democratic socialism, there were always clear tensions between the two. The neglected *transformative* view stressed the importance of democratic beliefs and ideology, whereas the elite conception minimized their significance. The transformative perspective highlighted the radicalizing, dynamic effects of movements that attempt to realize democratic values and act upon democratic ideologies, while his better-known elite model depicted democracy in static terms and as institutionally stable. Finally, the transformative concept emphasized the social and economic implications of spreading democratic movements and practices, whereas the elite framework held that democracy was simply an arrangement of *political* institutions. Any complete interpretation of Schumpeter's democratic theory must include both of these conceptualizations. Even his elite view is best understood not in isolation, as a complete and free-standing theory, but in relation to his theory of liberal capitalist development.[3] The elite model was not merely a descriptive conception, although it reflected what he thought was a clear-eyed view of politics. It took on practical, prescriptive significance as part of a sketch of a "democratic" socialist society in which the most dangerous democratic tendencies, from Schumpeter's deeply conservative standpoint, would be curbed. It was, in short, a reactionary response to the democratic social, economic, and political tendencies that he deplored.

Ignoring Schumpeter's attention to democracy's transformative significance has contributed to shaping a debate in which an elite conception is too often equated with empirical accuracy and value-neutrality and in which the critics of such a concept have been classified (at best) as idealists. Schumpeter's thought did not conform to these simple categories. The elite notion, though bound up in attempts to understand the social world, was at the same time deeply rooted in his own political aspirations. Meanwhile, his transformative conception took very seriously the social, political, and economic significance of attempts to realize democratic values by reconstructing institutions. Schumpeter, whose authority is so often cited on behalf of an elite view of democracy, clearly recognized the importance of comprehending democratic ideology and values as well as the potentially transformative social and economic implications of democracy.

Schumpeter (1946, 270) derided those who treated political "ideas and principles as if they floated in the air" instead of delving "into the historical conditions from which the ideas arose." In keeping with the spirit of this reproach, my essay focuses specifically on his work from the 1930s and early 1940s and relates it to the politics of the New Deal. Schumpeter elaborated his ideas on democracy and liberal capitalist development into their fullest expression while living in the United States during this period, and his later work can be understood partly in relation to his analysis of the democratic tendencies manifest at that time.

With good reason, some have characterized the 1930s as years in which labor democratized America (Freeman et al. 1992, 373). It was a time of tremendous labor mobilization, whose high points were the wave of citywide strikes in Toledo, Minneapolis, and San Francisco; the sitdowns that spread across the country from the Fisher One plant in Flint; the rise of activist industrial unionism; and the redirection of state power inherent in the passage of the National Labor Relations Act of 1935. Schumpeter took great interest in such developments and regarded them, in part, as elements of a social and economic democratization that, along with other tendencies, would ultimately bring an end to capitalism. He believed that certain "rationalizing, leveling, mechanizing and democratizing" tendencies inherent to liberal capitalist societies would eventually transform them into socialist ones (Schumpeter 1939, 697). Convinced that the advent of socialism was all but inevitable, but motivated by a desire to stave off the democratizing tendencies of New Deal America that he most deplored, Schumpeter sketched the outlines of an elite-dominated "democratic" socialism. His famous elite conception of democracy was a crucial component of this sketch.

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[3] I concur with Ashcraft's (1995, 2) view that Schumpeter's elite conception of democracy should not be "extracted from its context and considered as an independent or self-contained theoretical framework." My thesis, however, is a departure from Ashcraft's (1995, 28) conclusion that "Schumpeter never conceived of democracy as a set of beliefs capable of mobilizing masses of people to engage in political action" or that he "never undertook to explain democracy, as he did capitalism and socialism, in terms of sociological or historical tendencies."

SCHUMPETER'S SCIENCE OF SOCIAL TRANSFORMATION

Schumpeter's vision of social science provides an interpretive framework for reading his work on democracy, one that underscores the importance, for him, of the transformative conception. If the elite notion of democracy were abstracted from Schumpeter's broader writings on the subject, and if its chief features, considered in isolation, were assumed representative of his general approach to social science, then one might arrive at some misleading conclusions: that Schumpeter had little interest in relating political analysis to the problem of social transformation, that he generally found the analysis of ideology and beliefs to be of little value, and that he thought social scientists could generally gain great insight from separating political from social and economic facts and institutions by means of careful a priori classification. Each of these inferences would be mistaken. Throughout his career, Schumpeter moved from the realm of abstract "pure" economic theory to a richer approach to social science, informed by historical investigation and what he termed "economic sociology" (Bottomore 1992; Schumpeter 1954, 12–21; Shionoya 1990, 1991; Swedberg 1991b). His methodological development embodied a massive shift in the scope and ambition of his project, toward a social science capable of comprehending the development of societies from one form to another. This project—reflected in many of the titles of his mature works, such as "The Instability of Capitalism," "Wage and Tax Policy in Transitional States of Society," and "The March into Socialism"—involved an assessment of both the structural features and dynamic tendencies in a given form of society, making it a science of social transformation ([1928] 1951, [1949] 1976, [1948] 1991). Schumpeter's broader social science project placed great emphasis on ideologies and beliefs as catalysts of transformation. And, consistent with his longstanding view that the "social process" was "really one indivisible whole," that project cast serious doubts on the practical value of a priori distinctions among political, social, and economic realms (Schumpeter [1911] 1934, 3). If we are to read Schumpeter's work in a way that is consistent with *his* understanding of social science, *our* interpretive framework should reflect his emphasis on the problem of social transformation. Such an approach—mine in this essay—emphasizes Schumpeter's treatment of democracy itself as a socially transformative historical tendency and his integration of that concept into his theory of liberal capitalism.

The development of Schumpeter's views about the scope and method of social science had profound implications for how he viewed what we would call political theory. In essence, Schumpeter came to believe that arguments for or against particular political programs should be assessed in terms of the practical actions one could take to bring them about. And the assessment of practical actions in politics only took on meaning within the context of a larger social scientific project focused on the social, economic, and political tendencies of the time. In an illuminating review of Friedrich von Hayek's *The Road to Serfdom*, Schumpeter (1946, 269) wrote that while not necessarily bound to present a "policy of his own," a conservative thinker should at least present a "standpoint" from which a "politically effective program *could* be presented." That required above all that the conservative explain how existing social tendencies constrain and enable political action, and avoid dealing with ideas "as if they floated in the air" (1946, 270). On the one hand, since the "fundamental lines" of contemporary social development were "probably ineluctable," there was little point in imagining societies that were the perfect embodiment of conservative ideals; on the other hand, since "much in the way in which the ever-present change comes about depends on individuals and groups, abilities and volitions," it was at least worthwhile to focus attention on carrying out "[social] transitions with a minimum loss of human values" ([1941] 1991, 399).

DEMOCRACY, THE TRANSFORMATION OF LIBERAL CAPITALISM, AND NEW DEAL POLITICS

Schumpeter devised a comprehensive, multidimensional science of social development to deal with a crucial question: What would be the fate of Western liberal capitalism? His famous answer was that it already was evolving into a form of democratic socialism ([1928] 1951, 47–72; [1942] 1976; [1941] 1991, 339–400). Most accounts of this theory of social transformation focus on Schumpeter's argument that the individual entrepreneur was being displaced as a source of economic innovation, and with the demise of the entrepreneur, capitalism would fade, too ([1942] 1976, 131–4). The entrepreneur was, indeed, in Schumpeter's view, at the nexus of capitalist economic activity, and thus the entrepreneur's demise was a crucial component of his theory of capitalist collapse. His theory of the transformation of capitalism into socialism, however, involved not only the extinction of the entrepreneurial species but also the collapse of a set of feudal and hierarchical institutions and of a more recently developed capitalist "institutional framework" ([1942] 1976, 134–42). The passing of all these institutions—including the entrepreneurial function—he attributed to an underlying process: "the rationalizing, leveling, mechanizing, and democratizing effects of capitalist evolution" (1939, 697).

A crucial aspect of this description of "capitalist evolution" generally goes unrecognized: Schumpeter assigned a critical transformational role to democracy or democratization. It is, moreover, rarely noted that woven into Schumpeter's later discussions of the transformation from capitalism to socialism is an interpretation of New Deal politics. In describing these "rationalizing, leveling, mechanizing and democratizing" tendencies, Schumpeter drew heavily on examples from that era.

Also scarcely noticed is that a transformative conception of democracy was actually part of Schumpeter's thought from the late 1910s onward. Perhaps the clearest evidence is his "Sozialistische Möglichkeiten von heute" (The socialist possibilities of today), written while he was still living in Europe (Schumpeter 1920–21). There he borrowed from contemporary social democratic theorists and argued that an actual historical-sociological process, the spread of the council movement, in which workers sought to exercise self-governance in industry, would be a crucial factor in fundamentally reconfiguring social, economic, and political structures and enabling the transition to democratic socialism (Bottomore 1992, 37–8; Schumpeter 1920–21, 310, 338). Schumpeter's theory of liberal capitalist development and the transition to socialism progressed significantly after he wrote this essay and immigrated to the United States, in 1932, but he never ceased to emphasize a transformative conception of democracy as part of that theory.

Democracy and the New Deal

Schumpeter was not alone in seeing democratic ideas and democratization at the center of New Deal America. Scholars who have regarded that era as one in which labor democratized America have done so for several reasons (Freeman et al. 1992, 373).[4] It was only in the 1930s that U.S. workers secured the legal apparatuses allowing a semblance of democratic participation in the management of businesses. In addition, this democratization of the workplace necessitated a democratizing shift in power from courts to legislative bodies, because it was the courts and their application of common law doctrines that had protected the hierarchical nature of the production regime (Orren 1991, 8, 18, 29–40). Schumpeter's dismay at the breakdown of workplace hierarchy, his fierce denunciation of industrial democracy, and his objection to making the production regime subject to the "humors" of democratic political influence indicate that he had a broad awareness of these changes ([1942] 1976, 299–302; [1941] 1991, 359; [1948] 1991, 435).

Since at least the turn of the century, a wide range of Americans had used the term "industrial democracy"—or associated their ideas with it—in ways that were significant for the labor politics of the 1930s. The term was flexible enough to mean different things to its different users, of course, but its frequent use suggests an awareness of the idea of extending the beliefs and practices that underpinned political democracy into the sphere of social and economic relations, as well as of the ideological force of linking proposed social and economic reforms to democracy (Fraser 1989, 58–9; Plotke 1989, 127). Early in the century, the Industrial Workers of the World advocated "industrial democracy" (Foner 1965, 141–2). And a long line of socialists and radical liberals (to use John Dewey's phrase), associated at one time or another with the League for Industrial Democracy, used the term or proposed the democratization of industry; examples range from Upton Sinclair to Dewey to Norman Thomas, the socialist party presidential candidate in the 1930s (Dewey 1935, 34; Sale 1973, 677; Sinclair 1970, 422–9; Thomas 1938, 28). Schumpeter, who had witnessed the post–World War I flourishing of the council movement and who was familiar with the use of the term by English, German, and Austrian social democrats, noticed the disparate uses and complained that "Industrial or Economic Democracy is a phrase that figures in so many quasi-utopias that it has retained very little precise meaning" ([1942] 1976, 300n). But this complaint did not keep him from offering what he saw as a useful definition of "industrial democracy" or from grasping the sociological significance of the term ([1942] 1976, 300).

Liberal Capitalist Development, Rationality, and Democracy

Understanding the sociological significance, for Schumpeter, of movements for "industrial democracy" and similar tendencies requires a review of concepts that formed, in effect, the building-blocks for his theory of liberal capitalist development: the capitalist system, the capitalist order, tradition, rationality, and rationalization.

The Stability of the Capitalist System. Although he believed that capitalism would be replaced by a form of democratic socialism, Schumpeter did not think an economic crisis would kill capitalism. In particular, he did not regard the Great Depression as a sign of its imminent demise. In part this is because Schumpeter distinguished between the *capitalist system* and the *capitalist order*. By the former he meant only the capitalist economy, conceived as a set of "independent quantities," such as incomes, costs, and prices, all "mutually determining each other" ([1928] 1951, 50). By the latter, to which Schumpeter sometimes referred as the "civilization of capitalism," he meant a whole set of economic, social, and political institutions, beliefs, values, and practices, many of which aided the functioning of the capitalist system (Oakley 1990, 13; Schumpeter [1928] 1951, 49; [1942] 1976, 121–30).

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[4] Of course, to say that labor democratized America is only to say that it moved America in the direction of greater democracy. That the process was nowhere near completed by labor in the 1930s is clear, given just one of many possible examples: the continuation of systematic political exclusion and social oppression of African Americans. This is worth pointing out since, as Pateman (1996, 11, n2) has argued, "political scientists tend to gloss over how hard it has been to consolidate . . . universal suffrage" for women and racial and ethnic minorities around the world.

Schumpeter argued that the Depression, like most downturns, could be explained purely in terms of the economic system and was no more than an unusually bad slump caused by the "coincidence of depression phases" of several independent economic cycles; these were themselves normal features of capitalism (1939, 907; see also Brown et al. 1934, 3–21). More broadly, his theory of capitalist motion, first expounded in *The Theory of Economic Development* in 1911, implied that the capitalist system's cycles of boom and bust would never themselves lead to crisis in the Marxist sense. He had long argued, however, that his prediction of no inherent tendency toward crisis in the capitalist *system* was no more sufficient to predict the future of the whole capitalist *order* "than a doctor's diagnosis to the effect that a man has no cancer is sufficient basis for the prediction that he will go on living indefinitely" ([1928] 1951, 71). His belief that capitalism would be superseded by socialism did not rest on a prediction about the system, which he regarded as stable although cyclical, but on his prediction about the order, which he regarded as fundamentally unstable.

Rationality, Rationalization, and Tradition. According to Schumpeter, a process of rationalization was undermining the capitalist order. It should be said that his use of the terms "rationality" and "rationalization" was somewhat lax, but this reflected Schumpeter's agreement with what he viewed as a scholarly consensus holding that social change in modern European societies was linked to "rationalization." As early as 1920, he had described rationalization as a "stock theme" in contemporary sociology (1920–21, 318; my translation).[5] Despite his casual adoption of this terminology, however, it is clear that from early on he drew a very close link between rationalization and democratization, insisting, in the passage just cited, that the process of rationalization brought with it "democratization," "leveling," and "great instability for all power-positions" (1920–21, 318; my translation).[6]

Schumpeter's understanding of rationality shared some common features with the ideas of Max Weber and Karl Mannheim, both of whom had ties to Schumpeter.[7] All three drew a broad distinction between traditional and rational forms of social action; claimed to perceive a long-term historical tendency toward a preponderance of rational social action; described rationality in terms of self-consciously chosen means and ends; and conceived of rationality broadly enough to include social action in pursuit of diverse values, such as democratic ones (Alexander 1983, 25–9; Mannheim [1936] 1985, 34–6; Schumpeter [1911] 1934, 86–7, 1939, 99–100; [1928] 1951, 65–66, [1942] 1976, 121–30; Weber 1978, 24–6).[8]

The distinction Schumpeter drew between traditionally and rationally oriented action was of great importance in his theory of entrepreneurship, which stressed that entrepreneurs were able to break free from the customary social practices that bound most economic actors ([1911] 1934, 86–7; 1939, 99–100; [1928] 1951, 65–66). In his view, most actors relied on traditional, received norms and habitual, ingrained ways of doing things, practices linked to the social institutions that provided order and stability in society. Weber (1978, 25) argued similarly that "strictly traditional behavior . . . is very often a matter of almost automatic reaction to habitual stimuli which guide behavior in a course which has been repeatedly followed." Schumpeter contended that the maintenance of social structures was not to be explained by "rational considerations" but by "traditions, beliefs, religious beliefs, by such things as honor and beauty" ([1941] 1991, 359). Such beliefs and practices were often premised upon the commitment to "sacred or semi-sacred tradition" that could not be rationally defended ([1942] 1976, 144). Even liberal capitalism, like other forms of society, rested in part on "super-empirical sanction" and an "inherited sense of duty" as well as an acceptance of "classwise" rights, by which Schumpeter seems to have meant an almost feudal recognition of differential status in the social hierarchy ([1942] 1976, 127).

Rational social action—and the related historical process of rationalization—relied, by contrast, upon values, norms, and practices that had been scrutinized, interrogated, or questioned. Schumpeter ([1942] 1976, 122) argued that there had been a "slow though incessant widening of the sector of social life within which individuals or groups" relinquished inherited, traditional conduct and guided their actions rationally, "according to their own lights." Weber (1978, 25; emphasis added) stressed the same point in arguing that "*value*-rational action," in particular, could be distinguished by "its clearly self-conscious formulation of the ultimate values governing the action and the consistently planned orientation of its detailed course to these values." Mannheim ([1936] 1985, 35) noted that "rational decisions" require "the individual to free his judgements from those of others and to think through certain issues in a rational way from the point of view of his own interests."

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[5] "In der Soziologie von heute ist die Analyse dieses Prozesses unter dem Namen Rationalisierung des Lebens zu einem stereotypen Thema geworden" (1920–21, 318). It should be noted that there is no indication Schumpeter meant the term "stock theme" to be derogatory, for he himself adopted the terminology of "rationalization" uncritically in the same essay.

[6] "[Rationalisierung] ist der Prozeß der Unterwerfung des Menschen durch die Maschinen und die Technik überhaupt, ein Prozeß, der auch Demokratisierung und Nivellierung und daher, wie gesagt, große Unsicherheit aller Herrschaftsposition mit sich bringt" (1920–21, 318). There is a similarity here to the English phrases Schumpeter used almost twenty years later in writing *Business Cycles*, where he pointed to the "rationalizing, leveling, mechanizing, and democratizing effects of capitalist evolution" (1939, 697).

[7] Schumpeter's ties and debts to Weber are fairly well known (see, e.g., Bottomore 1992, 13–14, n20; Swedberg 1991a, 45–6, 1991b, 2, 88–93). Schumpeter knew Mannheim well enough that he reported having been contacted by Mannheim when the latter was seeking to escape from Nazi Germany and find work in the United States (Schumpeter 1933). Schumpeter subsequently wrote a few letters in support of Mannheim; furthermore, in developing his own theory of science and ideology, he later argued for the indispensable role of a "sociology of knowledge" approach in developing any such theory, citing Mannheim as the best writer on the subject (1933; 1954, 33; [1942] 1976, 11, n3).

[8] Sorting out everything Schumpeter wrote about "rationality" is difficult. In addition to these uses of the term, he also employed it in a narrower but more familiar sense, in connection with his writings on neoclassical economic theory and "methodological individualism," one tenet of which he sometimes referred to as the assumption of "rationality." I do not wish to suggest that these different uses are entirely distinct, but it is important to see the differences among them. In particular, although Schumpeter certainly was familiar with the use of "rationality" to mean the maximization of subjective utility, this was not its only meaning for Schumpeter (1991, 316–38). The point is worth emphasizing because of the extraordinary narrowing of the term in the social sciences in the last few decades.

In contemporary political science usage, accounts of social action that demonstrate its "rationality" are generally considered to be alternatives to accounts that seek to explain action by reference to values, ideals, shared social practices, and ideologies. It should be evident, however, that Schumpeter's use of the term generally did not imply such a distinction. Just as Weber (1978, 25, 26) had devised the concept of value-rational action (as opposed to simply instrumentally rational action) to include action systematically oriented toward "duty, honor, the pursuit of beauty, a religious call, personal loyalty, or the importance of some 'cause' no matter in what it consists," Schumpeter ([1942] 1976, 125–7) argued that "rationalization" could explain not only the efficient organization of the modern hospital but also the development of expressionist art, "the genesis of the modern lounge suit," and even democracy and social criticism.

For Schumpeter, the "rational" orientation toward self-chosen or scrutinized beliefs and practices led to a break with traditional, hierarchical beliefs and practices in modern society.

> The capitalist process rationalizes behavior and ideas and by so doing chases from our minds, along with metaphysical belief, mystic and romantic ideas of all sorts. Thus it reshapes not only our methods of attaining our ends but also these ultimate ends themselves. . . . On the one hand, our inherited sense of duty, deprived of its traditional basis, becomes focused in utilitarian ideas about the betterment of mankind. . . . On the other hand, the same rationalization of the soul rubs off all the glamour of super-empirical sanction from every species of classwise rights (Schumpeter [1942] 1976, 127).

And he insisted elsewhere:

> When the habit of rational analysis of, and rational behavior in, the daily tasks of life has gone far enough, it turns back upon the mass of collective ideas and criticizes and to some extent "rationalizes" them by way of such questions as why there should be kings and popes or subordination or tithes or property ([1942] 1976, 122).

As even these passages demonstrate, Schumpeter tied this "rationalizing" tendency very closely to political, social, and economic democratization.

The Capitalist Order: Tradition and Stability. Schumpeter contended that the stability of the capitalist order rested on the traditional social institutions, classes, beliefs, and practices that he considered to be its "buttresses": the bourgeois family, a political elite that could act in the interest of the bourgeoisie, and—a buttress of crucial interest here—a regime of severe discipline in the factory or workplace ([1941] 1991, 343, 359–60; [1942] 1976, 127, 135–6, 139–41, 157–61). He complained that nineteenth-century bourgeois liberals had all too easily forgotten that their society "was the product of force and of a discipline inculcated into the lower strata by the feudal predecessors of the business class" ([1941] 1991, 343; [1942] 1976, 214). He argued further that it was not rational agreement but the worker's traditional acquiescence to the belief that "his true reward was a satisfaction of employment, loyalty to the firm, and so on" that helped "keep society together" ([1941] 1991, 359). Such "loyalties" and "habits of super- and subordination" were "essential for the efficient working of the institutionalized leadership of the producing plant" ([1949] 1976, 417).

Although labor discipline is the capitalist "buttress" on which I will focus here, Schumpeter stressed others as well. He saw as particularly important the reliance of the European capitalist order on the political leadership of "feudal classes," but he never explained what application that claim could have in America or for his interpretation of the New Deal ([1942] 1976, 135–9). He emphasized the importance of a network of small proprietors who remained attached to the institutions of property and "free contracting," and he discussed the bourgeois family, whose maintenance and defense, Schumpeter argued, was the focus of bourgeois economic activity ([1942] 1976, 127, 139–41, 157–61).[9]

The Capitalist Order: Rationality, Democracy and Social Transformation. Schumpeter regarded liberal capitalism as unstable because, as we have seen, he thought it unleashed rationalizing social tendencies. He described two types of undermining tendencies: those that refashion the motives behind the "entrepreneurial function," which he saw as the source of capitalist dynamism, and those that tend to create radical hostility toward capitalism. But behind both of these he perceived the same process of rationalization (1939, 400).

Schumpeter explained that the beliefs and practices resulting from or part of "rationalization" were not logically necessary extensions of that process ([1942] 1976, 127). Democratic ideology and democratization, in particular, he regarded as contingent forms or results of rationalization, as possible but not necessary parts of the "rational" critique and dismantling of traditional social institutions and hierarchies. Rationalization could take different forms, in other words, but one of the most historically significant was the spread of "democracy" ([1942] 1976, 248–9). As Schumpeter understood the process, there were several strands tying rationalization to democracy. First, the rationalization of traditional institutions—insofar as it resulted in greater equality and freedom from old hierarchies—resulted in a more democratic society. Second, Schumpeter insisted that democratic ideas were, as a historical matter, a constitutive part of the rationalist civilization of liberal capitalism ([1942] 1976, 126) and that democratic political movements had been a crucial factor in the creation of that civilization. "Democracy," he argued, "presided over the process of political and institutional change by which the bourgeoisie reshaped, and from its own point of view *rationalized*, the social and political structure that preceded its ascendancy: the democratic method was the political tool of that reconstruction" ([1942] 1976, 297; emphasis added). Third, Schumpeter contended that the idealization of a human capacity for rational action was itself a part of democratic ideology. "The ideology of democracy as reflected in the classical doctrine rests on a rationalist scheme of human action and of the values of life," he claimed ([1942] 1976, 296).

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[9] I shall refer to the bourgeois family below, in connection with Schumpeter's scattered comments on feminism.

Despite these ties, Schumpeter did not attempt to reduce democratic ideology to rationality or rationalism. The process of democratic "rationalization" could never be understood without an immersion in the particular beliefs and ideologies that motivated the bourgeois political program and the meaning those ideas had in a particular historical context ([1942] 1976, 265–8). Thus, he briefly sketched the ideological materials that he thought had been joined together to form what he called the "classical doctrine of democracy" or "the classical doctrine of collective action" ([1942] 1976, 265).[10] And he related those ideas to the particular historical eras in which they took on meaning, acknowledging reluctantly that,

> in the first half of the nineteenth century, the oppositions that professed the classical creed of democracy rose and eventually prevailed against governments some of which . . . were obviously in a state of decay and had become bywords of incompetence, brutality and corruption. . . . Under these circumstances, democratic revolution meant the advent of freedom and decency, and the democratic creed meant a gospel of reason and betterment ([1942] 1976, 267).

Although Schumpeter had nothing but scorn for many facets of this democratic ideology, he recognized its historical significance—recognized that "action continued to be taken on that theory all the time it was being blown to pieces" ([1942] 1976, 249).

The Dangers of Democracy. But the rational critique of traditional norms and the democratic reconstruction of traditional institutions could not be limited to the realm of political institutions. And this was linked, Schumpeter claimed, to the downfall of capitalism:

> The reason, the deepest reason, why I think that capitalism won't survive is the rationalizing effect the system has on our minds, the effect the system has of doing away with everything traditional. For perfectly rationalized minds there is of course no justification for plenty of points, except by difficult arguments, unacceptable to the masses ([1936] 1991, 307).

Eighteenth- and nineteenth-century radicals had deployed democratic arguments in tearing down the feudal social and political order, he contended, without grasping the broader implications:

> The English liberal radical who looked upon the House of Lords as an anomaly and upon aristocratic cabinet ministers with distrust, was blissfully confident that the masses, when sufficiently educated and left free to vote, would recognize the excellencies of the capitalist system and keep it up of their own free will ([1941] 1991, 343).

What such bourgeois radicals failed to grasp was the way in which democratic arguments could become everyone's weapon.

> Formerly when people were dissatisfied with a pope or a king, they would revolt against the particular pope or king but they took the institution for granted. Now we ask ourselves, 'Why should there be such things as popes or kings?' The bourgeois asked this question. It did not strike him that the same question would be answered unfavorably to the capitalist ([1941] 1991, 360).

And so the rational critique of hierarchy turned eventually to what Schumpeter had identified as a bulwark of capitalism: labor discipline. As I have already noted, he argued that the "workman's readiness to obey orders was never due to a rational conviction of the virtues of capitalist society" ([1942] 1976, 214). Labor discipline was thus vulnerable to rationalist attack. He wrote in 1949: "Capitalist activity, being essentially 'rational,' tends to spread habits of mind and to destroy those loyalties and those habits of super- and subordination that are nevertheless essential for the efficient working of the institutionalized leadership of the producing plant" ([1949] 1976, 417). Since the rational critique that bourgeois democrats deployed in their reconstruction of politics could be deployed against capitalist hierarchies as well, Schumpeter argued that socialism and democracy were linked. "The ideology of classical socialism is the offspring of bourgeois ideology," he contended. "In particular, it shares the latter's rationalist and utilitarian background and many of the ideas and ideals that entered the classical doctrine of democracy" ([1942] 1976, 298–9).

Unlike many nineteenth- and twentieth-century liberals who made sharp analytical distinctions between public and private, and in whose work the family dropped out of discussions of social and political change, Schumpeter was closely attuned to the connections he perceived between the structure of the family and social, economic, and political transformation. According to his conservative analysis, one of the key buttresses of capitalist civilization, a "mainspring of the typically bourgeois kind of profit motive" and thus of entrepreneurial activity, had been the bourgeois family

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[10] Schumpeter's characterization of the political theorists that he groups together as proponents of "classical" democratic theory is highly misleading. As Pateman (1970) demonstrated, there is no "classical" theory of democracy uniting Bentham, the elder and younger Mill, and Rousseau. Furthermore, Schumpeter's characterization of Locke's political commitments, though no doubt on par for his time, would certainly today be regarded as misleading. The point is not that Schumpeter was a subtle reader of others' political theory. Rather, what he described as a process of "rationalization" was carried out by particular political movements, with varying ideologies and theories. His attempt to find unifying themes among these, insofar as it led him to posit the existence of a "classical doctrine," was misguided, but there is value in setting out a theory of the sociological and historical significance of democratization and democratic arguments in developing liberal capitalist societies.

([1942] 1976, 160).[11] But in Schumpeter's view, just as one of the forms of rational critique of inherited institutions and beliefs was democracy, another was the growth of "feminism" and the tendency for "family life and parenthood" to mean "less than they meant before" to both women and men ([1942] 1976, 157, and see also 127, 157–61). When rationalization reached "private life," he contended, both men and women were less inclined to have children; not having a private family realm to protect and develop, the typical (male) entrepreneur lost the will to accumulate and to fight in both the economic and political arenas ([1941] 1991, 374, 378). Thus, "the most obvious symptom of the decay" of capitalist civilization, Schumpeter argued regretfully, was "a fall in the birth rate in the upper strata of society" (1939, 401).

The New Deal and the Dangers of Economic Democracy. The spread of democratic ideology into the economic realm was at the center of Schumpeter's understanding of New Deal politics. Reflecting on the previous decade in a talk he delivered in the early 1940s, he said: "We have complete disorganization in 1933, a complete disbelief in standards, a disbelief both in the old social relationships and in the new ones" ([1941] 1991, 361). Worst of all, according to Schumpeter, that rational rejection of traditional loyalties meant the contemporary unraveling of labor discipline, which he saw as one of the most obvious processes visible in the New Deal era. In a passage that evoked what many American conservatives saw as the deplorable breakdown of social order in the 1930s—the general strikes of 1934 and the sitdowns of 1937—Schumpeter wrote that it had been a great mistake for the bourgeoisie to accept "equality in the political sphere" and to teach "the laborers that they were just as valuable citizens as anyone else," for that had dire social and economic consequences ([1942] 1976, 214). "Incessant change," he contended,

> . . . was bound to dissolve the discipline in the factory. . . . Gone are most of the means of maintaining discipline, and even more, the power to use them. Gone is the moral support of the community that used to be extended to the employer struggling with infractions of discipline. Gone finally is—largely in consequence of the withdrawal of that support—the old attitude of governmental agencies; step by step we can trace the way that led from backing the master to neutrality, through the various nuances of neutrality to backing the workman's right to being considered an equal partner in a bargain, and from this to backing the trade union against both employers and individual workmen ([1942] 1976, 214).

He continued in an explanatory footnote:

> Toleration amounting to encouragement of such practices as picketing may serve as a useful landmark in a process that has not run a straight-line course. Legislation, still more administrative practice, in this country is particularly interesting because the problems involved have been brought out with unequaled emphasis owing to the fact that change, after having been long delayed, has been crowded into so short a time ([1942] 1976, 214 n10).

In the light of these tendencies and his arguments for the necessity of strict labor subordination, Schumpeter was sharply critical of what he termed "industrial democracy." As one might expect from his understanding of traditional hierarchies, rationalization, and democracy, he contrasted industrial democracy with a traditional feudal order that he regarded as crucial to the capitalist workplace, and he drew an analogy between political and economic democratization. Industrial democracy meant "trade-union rule over industrial relations" and "democratization of the monarchic factory by workmen's representation on boards or other devices calculated to secure them influence on the introduction of technological improvements, business policy in general and, of course, discipline in the plant in particular, including methods of 'hiring and firing'" ([1942] 1976, 300n).

The process of democratization and destruction of traditional hierarchies was made much worse, Schumpeter argued, by the fact that the bourgeoisie possessed little of the political skill needed to resist the tendency—a problem for which he professed to see much evidence in New Deal America. "Perhaps the most striking feature of the picture is the extent to which the bourgeoisie, besides educating its own enemies, allows itself in turn to be educated by them," Schumpeter lamented in the early 1940s:

> It absorbs the slogans of current radicalism and seems quite willing to undergo a process of conversion to a creed hostile to its very existence. . . . In this country there was no real resistance anywhere against the imposition of crushing financial burdens during the last decade or against labor legislation incompatible with the effective management of industry ([1942] 1976, 161).

In fact, Schumpeter reserved some of his sharpest words for describing this phenomenon. He told an audience in 1941 that for the "executive of a company . . . it is better to be patted on the back and be told what a progressive individual he is and go off to Nice while there is a sitdown strike at home" ([1941] 1991, 378; see also ([1942] 1976, 214). In light of this weakness of bourgeois spine, Schumpeter could only reassure himself that in the socialist order he saw coming "there would no longer be . . . any well-meaning bourgeois of both sexes who think it frightfully exciting to applaud strikes and strike leaders" ([1942] 1976, 212).

New Deal Policy and Transitional States of Society. In addition to the direct effect on the workplace of the democratizing tendencies within liberal capitalism, Schumpeter argued that there was an indirect effect, mediated by the state and its economic and social policies. His most general assessments of New Deal programs and policies are best seen in this context. Understanding those assessments requires a brief detour through his notion of the state and its development. Schumpeter wrote, of course, before the renewed debate that has taken place since the 1970s concerning "theories of the state," but like many social scientists interested in the questions raised in that debate, he addressed the role of the state in liberal capitalist societies and did so in response to the way this problem had been established by Marxists. As against a fairly reductive statement of historical materialism—the contention that "social, cultural, and political situations and the spirit in which and the measures by which they are met, derive from the working of the capitalist machine"—Schumpeter countered that "the state does always reflect the social power relations even though it is not merely their reflection" (1939, 695–6; [1918] 1991, 138, n20; see also [1948] 1991, 429–30).

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[11] Schumpeter made, from a profoundly conservative standpoint, a somewhat contemporary-sounding critique of how the family dropped curiously out of view in neoclassical economics. "When we look more closely at their [the economists'] idea of the self-interest of entrepreneurs and capitalists," it becomes clear that "consciously or unconsciously they analyzed the behavior of the man whose views and motives are shaped by such a home and who means to work and to save primarily for wife *and children*," even though they never explicitly acknowledge this ([1942] 1976, 160). Of course, there was nothing of a critical or feminist edge to Schumpeter's observation.

This argument, in turn, was based on the view that social, cultural, and political institutions, though linked in an overall process to economic ones, could develop relatively independently (Schumpeter 1939, 700). There were "intact societies" in which "class structure, beliefs, values, attitudes, and policy are all consistent with each other," but there are "frequently" also situations in which "the class structure of a society and its civilization cease to correspond to each other" ([1948] 1991, 429). That was the case for most of the history of liberal capitalism, Schumpeter argued. In a 1918 essay, Schumpeter maintained that the origin of the liberal capitalist "tax state" had to be located in the political and social transformations of the early modern period. These began to mark a distinction between, on the one hand, civil society as a sphere of economic activity pursued largely out of self-interest, and on the other, the state as sphere in which the prince (and, later, citizens) pursued some conception of public interest, funded by taxes levied on private economic activity ([1918] 1991, 110).

Democratization entered crucially into the argument at this point. Schumpeter had described the first historical stages of democratization as a process of "political and institutional change by which the bourgeoisie reshaped, and from its own point of view rationalized, the social and political structure that preceded its ascendancy"—or more simply as the bourgeoisie wringing the state "from the hands of the prince" ([1918] 1991, 108, 111; [1942] 1976, 297). But long after the onset of democratization, Schumpeter argued, the European state remained a key component of an "amphibial" or transitional form of society, in which capitalism and the bourgeoisie were the most important forces in civil society, but in which the state retained some of its feudal characteristics, particularly its being staffed by the aristocracy ([1942] 1976, 134–9, 155; [1918] 1991, 108).

Schumpeter saw America during and after the New Deal as an example of a different sort of "transitional" state of society, reflecting a different phase of democratization and a different constellation of class forces. As I have shown, he argued that the United States in the 1930s was undergoing a partial democratic transformation of society, one already experienced by some European nations immediately after World War I. But that transformation did not completely undo capitalism. In Europe between the wars, Schumpeter contended, the policies of liberal capitalist states "adjusted themselves to a new social situation, which, whatever political party happened to be in power, substantially amounted to democracy in the sense of trade-union rule" ([1941] 1991, 346). In the meantime, however, the economy remained fundamentally capitalist, putting constraints on the range of action open to the state. Thus, there was not a perfect fit between the role of the state and the changing relative power of social classes:

> The admission of labor to responsible office and the reorientation of legislation in the interest of the working class were in a sense an adjustment to a new state of things. But, with the two exceptions mentioned [Russia and Italy], all nations nevertheless attempted to run their economies on capitalist lines, thus continuing to put their trust in an engine, the motive power of which was at the same time drained away by crushing taxation ([1941] 1991, 346–7).

Schumpeter believed that America reached the same position a few years later. "Consider the case of the United States," he wrote in the 1940s:

> The business class has lost the power it used to have, but not entirely. Organized labor has risen to power, but not completely. Labor and a government allied to the unions can indeed paralyze the business mechanism. But it cannot replace it by another mechanism. . . . [E]verybody checkmates everyone else ([1948] 1991, 430).

It was not just the growth of working class power that created this situation, Schumpeter argued. In the New Deal era, "intellectuals veered around to what they believed to be the rising sun. Definitely the intellectual interest went into the camp of social reconstruction" ([1941] 1991, 354; see also [1942] 1976, 145–55). Intellectuals "invaded labor politics" and "radicalized" them ([1942] 1976, 153–4). Meanwhile, in the absence of a strong bureaucratic class, intellectuals invaded the state as well (1939, 1048; [1942] 1976, 155). "In times of rapid expansion of the sphere of public administration, much of the additional personnel required has to be taken directly from the intellectual group," Schumpeter declared, adding: "witness this country" ([1942] 1976, 155). That fact, he contended, "explains why public policy grows more and more hostile to capitalist interests" ([1942] 1976, 154).

Under such circumstances, Schumpeter claimed, New Deal programs and policy had become anticapitalist in the sense that they threatened the functioning of the capitalist mechanism even while they could not replace that mechanism with another (1939, 1046–8; [1942] 1976, 64; [1941] 1991, 362, 395). "Capitalism produces, by its mere working a social atmosphere . . . that is hostile to it, and this atmosphere, in turn, produces policies which do not allow it to function" (1939, 1038). Schumpeter claimed that New Deal labor policy put the power of the state behind industrial unions, politicized the wage contract, and discouraged investment (1939, 1042–3; [1942] 1976, 214; [1941]

1991, 359, 435). He further claimed that New Deal fiscal policy diminished "'subjective' investment opportunity"—discouraged people from taking advantage of existing opportunities—and discouraged savings (1939, 1038–40). And, he contended, beyond a few early attempts to unfreeze markets paralyzed by the crash of 1929, many New Deal regulatory policies constituted a debilitating "indefinite threat" to capitalists (1939, 988, 1043–4).

SCHUMPETER'S ELITE CONCEPTION OF DEMOCRACY: A NEW LOOK

Presentation of Schumpeter's elite conception of democracy is often shorn of these historical and sociological references, which reveal the meaning it had for him as a deeply conservative response to the social tendencies discussed above. To leave out this context is to ignore the most obvious of textual clues, for even Schumpeter's chapter titles show that he was presenting not an abstract, timeless theory of "democracy" but a speculative sketch of what one form of "democratic" socialism might look like—a form of society in which the most alarming democratizing tendencies visible in New Deal America, in his view, would be subject to elite control. I have shown that Schumpeter thought a particular approach to political theory grew out of a social science directed toward examining social and historical transformation. He argued that it made no sense to offer a vision of a political and social ideal that was impossible to realize in light of existing social tendencies. Conservatives, he thought, had to assess those social tendencies and offer at least a "standpoint"—in this case, a scientific understanding of social structure and transformation—from which a "politically effective program *could* be presented" (1946, 269).

That argument helps explain the relationship between the transformative and elite conceptions of democracy in Schumpeter's work. The first was part of an analysis of existing social tendencies, according to which Schumpeter regarded socialism of some kind as all but inevitable, although its form was potentially tractable. The second was a description of a form of society in which certain of those social tendencies, especially the democratizing ones, could be managed. Since it would fall, in Schumpeter's view, to political and bureaucratic elites to curb these democratizing tendencies, the cultivation and mobilization of such elites was, as I will show, a major concern to him in the development of his elite conception of democracy.

The Origins and Defense of Schumpeter's Elite Conception of Democracy

Schumpeter employed both conceptions of democracy—the elite and the transformative—in works dating back to the late 1910s. It is important to emphasize, however, that the elite view always appeared in the context of a broader theory of social transformation, in which democratization played an important role. As early as 1916 Schumpeter was writing about elite—which he then called "Tory"—democracy, conceived explicitly as a political "technique" or program by which the aristocracy and other reactionary social forces could counter and control the democratizing tendencies in Austria-Hungary (Schumpeter 1985, 251–310, 1992, 361–75; Swedberg 1991b, 52). A few years later he made it clear that, given the broad processes at work in liberal capitalist societies, he thought elite groups could only dominate a democratic political system under specific, limited historical circumstances. He described a condition of elite democracy as just a phase in the democratic transformation of certain liberal capitalist societies (1920–21, 330). Similarly, years later, in *Capitalism, Socialism and Democracy*, Schumpeter reflected upon the possibility of an elite-dominated democracy as a response to what he conceived as the transformation and democratization of liberal capitalist society.

But in *Capitalism, Socialism and Democracy*, Schumpeter for the first time offered an elite conception of democracy as a general *definition* of what he now termed the "democratic method." The often-cited definition reads: "The democratic method is that institutional arrangement for arriving at political decisions in which individuals acquire the power to decide by means of a competitive struggle for the people's vote" ([1942] 1976, 269). In contrast to Schumpeter's portrayal of democracy as a socially transformative historical tendency, this elite conception of democracy as "method," considered in isolation, envisions democracy (1) under elite control and within narrow bounds, as opposed to dynamic and transformative, (2) stripped of any ideological ability to motivate political action, as opposed to being at the center of political movements to transform the state and society, and (3) purely political, as opposed to having implications for the structure of society and the economy.

In *Capitalism, Socialism and Democracy*, Schumpeter used three main arguments to define and defend this elite model. The first was to attack what he called the "classical" theory of democracy or the "classical" theory of "collective action," by which he meant a theory of how the people could act collectively so as to be sovereign, a theory that had, itself, motivated collective political action ([1942] 1976, 265, and see also 248–9, 250–68, 297–8). In the chapters devoted to his elite conception of democracy, Schumpeter set out a series of arguments meant to deny the possibility that the people could ever be sovereign (see esp. [1942] 1976, 250–68). These amounted to an attack on the political and cognitive abilities of nonelites. Thus, Schumpeter introduced Gustave LeBon's ([1895] 1981) profoundly antidemocratic theory of crowd behavior to support his contention that the "masses" are incapable of taking "purposeful responsible action" in politics ([1942] 1976, 256–7, 261).

The second argument was an attack on the idea that democracy itself—whether one means by that democratic self-rule or democratic ideals, such as equality—could or should be an end. Democracy, Schumpeter claimed, could only be "a political *method*, that is to say, a certain type of institutional arrangement for arriving at political—legislative and administrative—decisions and hence incapable of being an end in itself.... And this must be the starting point of any attempt at defining it" ([1942] 1976, 242). Schumpeter's defense of this claim is interesting, for he did not contend that, historically and empirically, no one had ever held democracy as an end. (In fact, as I have shown, he expressly admitted this.) Rather, he equated a belief in democracy or democratic principles with paying thoughtless homage to whatever outcome might result from majority rule, and this he called absurd and dangerous.

> No doubt one might conceivably hold that, however criminal or stupid the thing that democratic procedure may strive to accomplish in a given historical pattern, the will of the people must prevail, or at all events that it must not be opposed except in the way sanctioned by the democratic principles. But it seems much more natural in such cases to speak of the rabble instead of the people and to fight its criminality or stupidity by all the means at one's command ([1942] 1976, 242).

According to this openly elitist argument, one should regard democracy as a method because it would be foolish and dangerous to trust politics to the masses.

Schumpeter's third argument was the claim that the "method" of democracy was "competitive leadership" ([1942] 1976, 284). This form of democracy did not mean popular rule but the "rule of the politician" ([1942] 1976, 243, 285). Here is the ostensibly hard-headed core of Schumpeter's elite conception of democracy: the "recognition" of "a distinct professional interest in the individual politician and of a distinct group interest in the political profession as such"; the inference that "the democratic method produces legislation and administration as by-products of the struggle for political office"; and the consequent comparisons of democracy with economic market activity ([1942] 1976, 285–7). A political elite capable of using "democratic" strategies and institutions to dominate politics is the essence of this model. But this was not the only elite group needed to ensure order in Schumpeter's vision of "democratic" socialism.

Schumpeter's "Democratic" Socialism

A vision of a "democratic" socialist society holds a central place in *Capitalism, Socialism and Democracy*, and it surely holds the key to understanding why Schumpeter introduced his elite conception of democracy in that work. Even his own theory of capitalism's evolution toward socialism leads one to expect that democratic socialism will include democratized workplaces. But Schumpeter, as we have seen, was opposed to industrial democracy. Thus, in order to construct *his* conception of "democratic" socialism, he placed a simple, abstract definition of the "democratic method" alongside a simple, abstract definition of socialism: state direction of the means of production. The juxtaposition of these definitions allows him to conclude that "between socialism *as we defined it* and democracy *as we defined it* there is no necessary relation" ([1942] 1976, 284; emphasis added).[12] Given his definitions, "democratic" socialism could only refer to a society that happened to combine a political system of "competitive leadership" with state control of the economy. Logically, this juxtaposition of abstractions means that there need not be any economic or industrial democracy in a system of democratic socialism.

But Schumpeter, led by his own intensely conservative inclinations, went farther than this. I have already noted his vehement opposition to industrial democracy. He declared that "no responsible person can view with equanimity the consequences of extending the democratic method, that is to say the sphere of 'politics,' to all economic affairs" ([1942] 1976, 299). Furthermore, he claimed that his view would be shared by the bureaucratic elites of the future "democratic" socialist society. As a result, he argued, "much of this economic democracy will vanish into thin air in a socialist regime" ([1942] 1976, 300n). Schumpeter explained a few pages later: "Effective management of the socialist economy means dictatorship not *of* but *over* the proletariat in the factory.... As a matter of practical necessity, socialist democracy may eventually turn out to be more of a sham than capitalist democracy ever was" ([1942] 1976, 302). Schumpeter even noted that bureaucratic elites might be able to use factory discipline to "restrict" the political expression of workers ([1942] 1976, 302). His countervailing admission that workers might be able to use their political rights to soften factory discipline seems half-hearted, at best, in light of his denial that the masses possessed the ability to take "purposeful responsible action" [1942] 1976, 261, 302). In any case, with obvious relish he claims that economic managers in the socialist society he envisions will not brook interference from "fussing citizens' committees or by their workmen" ([1942] 1976, 299). Workers' resistance will be broken, so that "every comrade will realize the true significance of restiveness at work and especially of strikes" as "nothing else but anti-social attacks upon the nation's welfare" ([1942] 1976, 211–2). Clearly, the key to staving off economic democracy is a bureaucratic elite committed to such views—the second of the two elites for which Schumpeter envisioned essential tasks in his "democratic" socialist society.

A Political Program?

It is not generally appreciated just how extremely conservative, even reactionary, a picture of Schumpeter emerges from this long-available evidence: from his fierce attack on the grounds for esteeming democracy and from his willingness to accept elite-bureaucratic domination, even "dictatorship," in the factory as stabilizing elements of the coming "democratic" socialist society. These alone indicate a political vision that tilts alarmingly toward authoritarianism. And in light of recently uncovered evidence that Schumpeter privately harbored Nazi sympathies, there is little basis for responding with the claim that his innermost political sentiments were actually moderate.[13]

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[12] Clearly, Schumpeter here was making a different type of argument than he had used elsewhere to demonstrate the many sociological and historical links between democratizing tendencies and the advent of socialism.

Still, the political program actually visible in Schumpeter's later public writings and speeches, though reactionary, is tentative and pessimistic; it amounts to an examination of the methods for stemming the democratic tide and the possibility of cultivating, educating, and supporting the elite groups Schumpeter hoped would serve this purpose. He had long been working on a theory concerning the circulation of elites. As early as 1911 he argued that classes rise and fall because of their ability or inability to carry out necessary functions in particular forms of society ([1927] 1991, 230–83). But while Schumpeter thought a form of "democratic" socialism was possible in which democratic tendencies would be curbed by elite domination, he apparently never reached a satisfactory conclusion as to how that form of society was to be achieved. He was not at all certain where or how the needed elites could be found, or how they could be organized and mobilized.

Schumpeter argued that in Europe during the stable era of liberal capitalism, the feudal aristocracy had provided political leadership. But the processes of rationalization, leveling, and democratization had or would destroy that elite, as those processes would soon destroy the bourgeois elite. Thus, in a neglected passage of *Capitalism, Socialism and Democracy,* Schumpeter wrote that "socialism" of the kind he had described and prognosticated—characterized by the dominance of a political elite and an economic-bureaucratic elite—"has no obvious solution to offer for the problem solved in other forms of society by the presence of a political class of stable traditions" ([1942] 1976, 302). It was thus "idle to speculate" about the "quality" of the class that might emerge to fulfill this function. Moreover, the existence of economic leadership in the "democratic" socialism Schumpeter envisioned was equally problematic. He contended that European bureaucracies might be converted to service as the managers of a socialist economy ([1942] 1976, 155), but the United States lacked a bureaucracy ready to take on this role (1939, 1048; [1942] 1976, 155).

Schumpeter's preoccupation with this problem provides more evidence of the motivations structuring his democratic thought. In 1936 he delivered a lecture entitled "Can Capitalism Survive?" at the U.S. Department of Agriculture Graduate School. As Schumpeter surely knew, this was as likely a place as any in the United States to find a bureaucratic elite potentially convertible for service in a socialist society—even in the aftermath of the *Butler* and *Schechter* decisions.[14] He was invited to lecture on an issue of his choice concerning economic technique (Sarle 1935). Instead, he chose to address the transformation from capitalism to socialism and the demands it would place on bureaucratic elites. Schumpeter crowned his lecture with the following observation:

> Of course no class has a more heavy responsibility to bear than a civil service class. They have to understand which way things are [going], for this is essential in being good servants to the public. . . . I would say [that] the success of the other schemata of life and of the workability of noncapitalist forms of life rests largely with the success to build up an efficient civil service of well-trained, good types, removed from direct political influence, and so on. Many things might be easier if such a class can slowly rise ([1936] 1991, 306).

This was a problem that greatly concerned Schumpeter in the last years of his life: the emergence, cultivation, and mobilization of conservative elites. Interpreting his elite conception of democracy in the context of his broader concerns about the development of socialism should help make it clear that *Capitalism, Socialism and Democracy*, like this speech at the Department of Agriculture, was itself a deeply conservative bid to solve that problem—in particular to instill an elite audience with a sense of the "way things are [going]" and lay out the potential grounds for the management of the most dangerous of contemporary social tendencies.

CONCLUSION

What can be gained from restoring the complexity and internal tensions of Schumpeter's thought on democracy? Part of the answer lies in the great influence Schumpeter had on the development of thought about democracy in the last fifty years. But that alone is not sufficient to justify the effort. Schumpeter's transformative conception of democracy has value in its own right. For Schumpeter, it helped him make sense of the development of liberal capitalist societies, even though he clearly deplored spreading democratization. For radical and participatory democrats—proponents of a democratic restructuring of society—it may help connect their program to a historical and sociological conception of democracy, viewed as a transformative force in society. From either standpoint, it is evident that a great deal has been lost when social scientists and political theorists abstract Schumpeter's elite conception from the whole of his thought on democracy and social transformation. Both the force of arguments for democratic participation and the actual historical significance of struggles over the scope of inclusion and participation are lost when democracy is viewed merely as an institutional arrangement fostering elite competition. So, too, are the importance of ideals such as freedom, equality, and human development—along with their significance in motivating and gathering adherents to democratic movements. Again, the possibility and significance of demands that social and economic institutions be democratized are simply lost to a conception that defines democracy, a priori, as an arrangement of political institutions. And all these losses are aspects of a broader one: the loss of the ability to view democratization as a transformative historical tendency in liberal societies, a tendency linked to concrete battles for expanded participation, arguments for the realization of democratic values, and movements to democratize and restructure social and economic institutions.

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[13] Robert Loring Allen and Richard Swedberg deserve considerable credit for bringing much of this evidence to light: such as Schumpeter's diary entry admitting that his feelings were "around Hitler" in 1932; a 1932 speech in which he suggested to German economics students that they might consider joining the Nazi party; and private comments and diary entries suggesting Schumpeter could "understand" Hitler and Nazism, would not condemn them in light of German social and political tendencies, and thought Hitler was viewed unfairly by Americans (Allen 1991, I, 284–7, 288, II, 58, 66, 91–2, 101–3, 138–9; Swedberg 1991a, 28, 86, n90, and 1991b, 148–50, 216). An adequate assessment of this evidence would require placing Schumpeter in a European context, rather than the American context emphasized here. But even in the absence of such an assessment, it seems mistaken to me to try to minimize the significance of this evidence by appealing, as Allen does, to Schumpeter's alleged political indifference or naiveté (Allen 1991, I, 284, 285, 286).

[14] In *United States* v. *Butler*, 297 U.S. 61 (1936) and *Schechter Poultry Corp. et al.* v. *United States*, 295 U.S. 553 (1935), the Supreme Court struck down the Agricultural Adjustment Act (AAA) and the National Industrial Recovery Act (NIRA), respectively. Both were far-reaching New Deal attempts at government economic intervention. Unlike NIRA, the AAA was quickly replaced by legislation that would accomplish the same goals without risking constitutional challenge.

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The Boundaries of Public Reason

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Modern versions of public reason occasionally must address disputes so profound and divisive that the adjudicative powers of reason cannot resolve them on the expectations of liberal governance. The beliefs underlying these disputes represent the legitimate pluralism of the liberal state, but they also reproduce within *the languages of public reason the same divisive views that the liberal state must manage in the larger society. The effects of divisive beliefs can be mitigated, however, with a noncomputational version of public reason that allows collective terms to dominate simple merit adjudication. This type of reasoning requires a survey of considerations beyond the merits of the case at hand and opens public reason to the more general needs of the political society.*

The main burden of public reason in the liberal state is to reconcile claims originating in political differences among persons who may have nothing in common except membership in the political system. One of the more prominent exemplars for such reasoning is a supreme court in a constitutional regime with judicial review (Rawls 1993, Lecture VI). It is easy to see why. Deliberation is the preeminent mode of rational dialogue in a legal forum. Claims enter legal domains thick with reasons, justifications, descriptions, and in general with attachments that encourage reflections and judgments within a framework of accepted rules of inference, evidence, and argument. Even though all citizens in a democracy must be prepared to use reason in public matters, judicial forums seem more effective in modeling basic expectations for reasoning in liberal settings. These expectations include the thought that the state can be reasonably independent of partisan or divisive values. Public reasoning in its judicial mode is expected to produce impartial conclusions and to achieve the political reconciliations needed for consensual governing in liberal democracies by relying on values that everyone would reasonably endorse.

The tasks required of public reason in realizing these expectations can be demanding. Suppose the radical starting fact of political liberalism: Members of disparate communities must find a way to govern their relations with one another even though their values are hostile or incompatible. If the reasoning of each community under this condition yields mutual opposition, not accord, then rational compatibility must be sought in resources outside community dialogue. Public reasoning is designed to provide these resources. A fully visible set of political rules and principles is to extend across the pluralism anticipated, and even celebrated, in liberalism. The liberal state governs on the expectations that persons have incompatible beliefs and that a shared form of reasoning can adjudicate the disputes that follow differences in beliefs.

The immediate difficulties of this program of governance are evident in any survey of the stages of deliberation. It is an axiom of decision theory that one must be rational in at least two ways: in selecting and in using the appropriate decision rules. The exercise of public reason must shield both of these areas of reflection and choice from the simple dominance of community beliefs if governing languages are to be impartial, stable, and generalizable. But the choice and use of decision rules, controlled by inclinations toward risk and the schedule of goods at issue, are notoriously open to particular and even idiosyncratic beliefs. Conditions of risk and uncertainty permit the signature conclusion of contemporary decision theory: One can be rational and wrong.[1] The influence of a variety of values on the stages of reasoning also invites the more severe restriction: A set of persons can all be rational and right and still be in a rational dispute with one another because of the heterogeneity of evaluative criteria. This possibility introduces the prospect of a limiting condition: Public reason may have to reconcile differences among communities when communities may be able to control the uses of reason.

This paper examines the reconciling powers of a merit form of public reason, by which is meant reasoning concerned with claims entered in a deliberative space shielded from historical or political influences. This type of reasoning is revered in all liberal traditions and is found in many liberal practices and institutions. The sequestering of juries, life tenure for justices, and legal rules of evidence are examples. Liberal settings are generously endowed with designs that aim to insulate deliberations from considerations outside the merits of issues and problems. These efforts to secure judgments that are blind to more general contexts are

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[1] Remember that there are no rational sanctions assignable to either risk-averse or risk-favorable inclinations. Note also that I am using the standard demarcations among conditions of certainty, risk, and uncertainty. One may ask why the use of formal decision rules is to count as rational. One may answer that formal systems provide a coherence to both preferences and actions, as does a narrative (although in other ways). Velleman (1993) draws this analogy.

attempts in liberal traditions to satisfy objectivity and impartiality. One goal of this paper is to demonstrate, or at least persuade, that this version of public reasoning is unintelligible in conditions of what I call deep pluralism, and that the model of reasoning more appropriate to these conditions is one that allows deliberations to be affected by the political arrangements in which claims are formed and presented.

The effects of this type of discussion extend beyond the confines of pure theory. Any theory of public reason has obvious implications for institutional arrangements. A merit version of public reason yields those tiered arrangements that seem to be required in adjudication: the ideas of claimants, public space, and impartial judges that sketch the institutional framework of political liberalism. Also found in merit arrangements is the expectation that reasoning will lead to convergence, not divergence: The exercise of intuition, common sense, science, and reasonableness will incline members of the political order to support the rules and procedures of the political domain (Rawls 1993, especially Lecture III). In this way the state and its member communities occupy separate levels connected by the common thread of public reason. This close relationship of merit to the arrangements of the political domain suggests that any impressive changes in theories of public reason will require a reordering of political institutions as well.

The discussion begins with a series of thought experiments on the practical operations of public reason. I argue that it is impossible to reason to impartial or fair conclusions with two types of speakers in political settings, largely because of the scope and depth of the disagreements they represent. I also maintain that incompatible beliefs represent the legitimate pluralism of the liberal state and reproduce in public reason the disputes that the liberal state must manage. I further argue that the divisive outcomes of public reason cannot be mitigated by excluding comprehensive doctrines from public space. The routine connections between moral and political reasoning make it unlikely that the political domain can be independent of moral influence. The task is to identify those forms of public reasoning that combine moral and political considerations. The model of public reasoning which succeeds in this assignment is a noncomputational reasoning that allows collective terms to dominate simple merit adjudication. These terms require a survey of considerations beyond the merits of the case at hand, and they open public reason to the more general needs of the political society.

SPEAKERS IN LIBERAL DISCOURSES

The languages of liberalism, like all languages, must be interpreted in their uses in public reason. The most benign interpreter is one who is a native speaker and employs the principle of sincerity in stating what terms mean. Disputes between these types of speakers originate in legitimate differences in interpreting texts. Such disputes can be profound and sometimes are among the more acrimonious in political societies. Abortion disputes, for example, are mainly over the sense and reference of the terms *person* and *harm*. Even when sincerity is conceded to the disputants, the disagreements can continue, for the languages central to the dispute may admit rival and reasonable interpretations among native speakers.

In many ways disputes that are produced on common languages and ontologies are the more mysterious in liberal settings, especially when they are intractable. That competent and reasonable persons (those who want to find an agreement) sharing fundamental perspectives can be in an intractable dispute is a more distressing failure for liberalism than are the problems occasioned by radical differences and defections. It suggests a failure of reason on those conditions where it should be more likely to succeed. It also seems to compromise the effectiveness of consent, which is a central term in liberal governance.

A second type of speaker can be labeled a radical. This appellation is offered here as a neutral or nonevaluative designation in a certain range of disputes. Occasionally, disputes can be traced to the fact that persons have neither a common language nor ontology for negotiating an agreement. Disputes between secular and religious communities, for example, may not be over the meaning of shared terms but the appropriate languages to invoke (e.g., whether God is a relevant consideration) and the scope and nature of the real. Radical status in this special sense means that the disputants may not speak the same language or share understandings of reality, even when they are members of the same political system and are all native speakers of the dominant language. The existence of this type of speaker leads to indeterminacy of translations and good faith efforts at communication across disparate communities (Quine 1960, 1969).

A third type of speaker is one who has defected from the political dialogue that negotiates disputes. Such speakers may have a common language and ontology, and may even agree on the sense and references of governing terms, but one or more of them has opted out of the cooperative game. Communication problems are primarily framed as some combination of logic and fairness in these types of dispute. In moral philosophy the proposed resolutions have usually amounted to appeals to defectors to reenter the cooperative game. Various sameness rules, for example, invite a defector to consider what would happen if everyone did what s/he is doing, or what it would mean if s/he continued to defect when others who are relevantly similar are members of the game, and so on (Hare 1965, Singer 1963). Nonplayers in many ways pose the more traditional and uncomplicated problem of political dialogue, namely, that some speakers who are competent and who share basic perspectives nevertheless choose not to comply with the rules and principles of governance. One example of this type of defection is the "free rider" problem in both politics and economics: An individual who has consented to a collective action but decides to consume the products of this action without assisting in the production costs. In cases of defection, individuals fail to cooperate even though they are members of the political system and have accepted common understandings of what membership requires. Coercive measures can be selectively applied to such persons once the appeals to them have been exhausted.[2]

Let us imagine an ideal forum in which sincere and radical speakers are in a dispute with adversaries of their same type, and the defector is in a dispute with cooperative players.

Sincere Speakers

Sincere speakers may be in a dispute with one another on any of a number of grounds, since a common language and shared political (or even moral) principles are no guarantee of lasting harmony. Abortion disputes illustrate some of the extreme divisions that can occur even among reasonable persons. Any tentative (and incomplete) framework for the many ways in which persons reason on political matters allows that rational persons have clusters of beliefs. These clusters are arranged in different ways on different sets of issues and problems, and the inventory and arrangement of beliefs in each cluster vary with changes in circumstances. Some of what we include in sets of rational beliefs must include the ordering principles that arrange clusters and beliefs. At least occasionally an ordering will yield a representative or dominant belief. This yield typically follows reflection on a number of other considerations and often is a derivative belief. The belief that abortion should be proscribed legally, for example, occurs (when it has rational standing) after a chain of reasoning on other matters, including the biological gestation of life, various legal principles and rules, and the membership criteria for human communities.

In the well-known story, contemporary participants in abortion disputes accept roughly the same set of moral and political principles but disagree over the references of the principles. Both prolife and prochoice advocates subscribe to the basic features of the liberal program, which includes an emphasis on individual rights and liberties, the *harm* thesis developed in Mill's libertarian philosophies, and the importance of individual life. Both accept the practical conclusion of charging the state with the protection of life. Both acknowledge the reality of human life from birth to death. Many prolife advocates, however, extend the protective role of the state to embryonic life from fertilization to birth. Prochoice assigns partial protection in stages to embryonic life, usually beginning with viability, and resists the full state protection of life until birth. Prolife demands that the law protect fetal life in early gestation on the grounds that it is in all important moral respects identical to life after birth. Prochoice, by either denying the human status of early gestation or granting priority status to a woman's dominion over her body, allows other parts of the liberal program to dominate, in this case the discretionary authority to control one's own body.[3] The practical outcome of each set of interpretations and arguments is familiar: For the prolife side, no abortions, and for the prochoice side, freedom of choice on abortions for women in at least the early stages of pregnancy.

What can public reason do to resolve such a dispute? In disputes among speakers who share the same moral and political program, the problems include selecting and defining the principles to use in negotiating a resolution. In abortion disputes the beliefs of the adversaries determine (in theory) the relevant principles. If the Fourteenth Amendment's requirement of due process were to be used to adjudicate abortion, then the effective result would be to deny one of the two central beliefs in the dispute *before* deliberations begin. If, for example, sexual equality defeats a prolife view, then one assumes either that the fetus is not human life or is subordinated in importance to a woman's dominion over her body. Neither assumption accepts the preeminence that prolife advocates attach to early stages of human life.[4] It is also impossible to assimilate abortion to other affiliated principles without compromising prolife beliefs, the crucial one of which is that *another* is present in the body of the woman during pregnancy. One cannot, on this belief, cede control to the woman to decide on abortion without introducing morally controversial parallels of euthanasia decided by others and of homicide. The deliberations of due process turned in other directions would simply order the rival concepts differently. To require legal justifications for abortions in the first trimester is hostile to the prochoice argument that women have full discretion on whether to continue or terminate a pregnancy. Even to explore reasons for ending fetal life in early abortions is already to concede some of the prolife agenda. In theory, governing principles are authoritative over practices. In this case public reason seems to be a simple product of each practice.

Clarifying the meaning of relevant principles is also not very helpful. Abortion disputes do not turn on definitions of liberty and equality, or even harm. The disputes begin on the concept of *life* itself, in particular when human predicates can be assigned to gestational life. The assignment of the predicates influences the relevance, orderings, and interpretations of other principles. Dworkin (1993) has argued that all sides on abortion agree about the sanctity of human life but disagree about the relative moral importance of natural or human dimensions of life. The natural in Dworkin's discussions is more or less biological, the human a notion of (human) investment in life. On these terms the abortion debate is in part over whether the frustration of a biological life is sometimes justified in order to avoid frustrating a human contribution to life or to other persons' lives. The key feature of the disputes is that reasonable people can disagree on the meanings and orderings of various understandings of life. No compelling reasons justify either prolife or prochoice assignments (even on Dworkin's optimistic expectations for rational dialogue). The moral indeterminacy of the early stages of human gestation and the inherent reasonableness of rival orderings of natural and human dimensions of life preclude any deliberative closure of the dispute. It is not clear how public reason can resolve this kind of dispute. All deliberation on abortion seems to start with an acceptance of either prolife or prochoice beliefs, not conclude with them as a reasoned outcome.

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[2] Competence is important in this argument. The mental powers needed for *mens rea* must be demonstrated before punishment is justified. Also, the regime must meet at least rudimentary tests of fairness. See Dagger (1993). Allowances must also be made for legitimate dissent.

[3] Thomson (1971) argues that granting human standing to the fetus still does not override the right to control one's own body. I have found in my research and conversations among prochoice people exactly such an overriding priority for the woman's discretionary powers, whether or not human life can be claimed for the embryo or fetus.

[4] For versions of due process that turn on sexual equality, see Calabresi 1985 and Tribe 1988.

Rawls (1996, lv–lvi) has more recently acknowledged that "disputed questions, such as that of abortion, may lead to a stand-off between different political conceptions, and citizens must simply vote on the question.... the outcome of the vote is to be seen as reasonable, provided all citizens of a reasonably just constitutional regime sincerely vote in accordance with the idea of public reason." This may be one way to resolve otherwise intractable disputes, except for the reality of strategic voting (along with the difficulty of specifying "sincere" voting) and the chronic breakdowns in arithmetical composition rules documented in collective choice theory (see overviews in Riker 1983, dissents and qualifications in Green and Shapiro 1994). The use of voting as a supplement in public reason is complicated in yet another way by the recognition that individuals may be members of different senses of a political system (indicated by Tsebelis 1990), a prospect more likely in strong pluralism. The possibility of multiple political contexts in the "same" political society undermines the notion that all citizens in a liberal democracy subscribe to the same understanding of voting, no matter how minimal this understanding may be. Voting in these disjointed conditions is hardly in any kind of accordance with the shared deliberative standards of public reason.

The disputes between prolife and prochoice advocates affect even the role of information in rational decisions. Abortion counseling illustrates this influence. Prochoice has consistently opposed prolife counseling of women contemplating abortion on the ground that it unduly influences women to forgo abortion (even given that the need to ensure informed consent may require prolife information), an argument accepted by the Supreme Court in *Thornburgh* (1986). For years prolife groups were successful in eliminating federal funds for family planning clinics that counseled pregnant women on abortion as an alternative to carrying to term. The dispute over the type and scope of information provided in counseling can be traced back to metatheories of morality. Prolife wants detailed (perhaps moral) material on gestation included in the counseling package on the ground that any complete description of abortion must include such facts, while prochoice wants only minimal (perhaps nonmoral) data provided to women on the assumption that the woman deciding whether to have an abortion assigns moral features to the act. Each position forces us to reconsider traditional views on the relationship of information and rationality.

Radical Speakers

Disputes between radical speakers extend the range of indeterminacy. To see these difficulties, think of a disagreement between a secular physician and a Christian Scientist over medical care for the latter's infant. To a Christian Scientist there is no therapy, no cure, because there is no illness. All that is real must be good. Illness, as an apparent and even obvious bad, must be illusory. Prayer and meditation will extinguish the illusion by bringing one's beliefs into accord with the real, which is God's creation. For a secular practitioner of contemporary medicine, illness is real. It is a condition of the person resulting from a variety of causes, including viruses, bacteria, malnutrition, trauma, genetic breakdowns, and other factors. Illness can respond to therapy, sometimes dramatically, at other times slowly and only marginally, sometimes not at all. The medical practitioner uses physical and mental means to restore health and prolong life. The Christian Science practitioner prays and meditates to change attitudes toward purported illness (Eddy 1971, Peel 1988).

Christian Science and secular medicine contain beliefs that are adjacent and so may seem to overlap. Physicians are more readily inclined to accept effects between beliefs and health, so that exclusive preoccupations with the body (as a physical unit) are not so prominent in therapy today. But this shift in attitudes is only a parallel belief. Christian Science begins with a rejection of "mental" cures and the acceptance of a reality that does not contain illness. Beliefs cannot affect that which does not exist. The core differences between Christian Science and secular medicine are ontological. Each practice derives from an understanding of reality that in all vital matters excludes the rival ontology.

The differences between these types of radical speakers affect concepts of reason. All empirical versions of reasoning, which are prominent in medical practices, use information that is falsifiable and the exclusive ground for deliberations. On the first point, metaphysical knowledge, understood as knowledge that exceeds the boundaries of naturalism, is excluded from reasoning. Only naturalistic knowledge is allowed, though the expressions of knowledge are various and include descriptions, mathematical formulations, speculative hypotheses, and natural laws. On the second point, the reasoning that leads to conclusions (directives and explanations mainly) is confined to the domain of what can be known, however the known is defined. Conclusions justified by reference to what cannot be known are illicit. This secular version of reason is at odds with the spiritual (almost Platonist) approach to reasoning by a Christian Scientist, who relies on perspectives that may be known only by God and who accepts a controlling status for the unknown, in the sense that reason depends more strongly on what is not known (by humans) than what is known.

Naturalistic and spiritual accounts of human experience often rely on common assumptions and employ similar research methods. That we must test descriptive statements with evidence drawn from only partial knowledge of our environment, for example, is a working premise endorsed by both skeptics and believers. Inductive statistics are often the preferred methods for those subscribing to this premise. But even shared research programs can still address different realities and produce radically different conclusions. The existence of God and the appearance of God's handiwork on earth are empirical claims in religious dialogues, as are beliefs in at least partial knowledge of higher plans and the presence of various types of spiritual figures, sometimes including God. The naturalist describes and explains, and sometimes expands, an empirical world that has been emptied of extraordinary individual powers and spiritual presences. These differences can also produce divergent research programs. Consider the languages that are relevant to the dispute over therapy. Foundational concepts of health, life, and illness, terms such as cause and effect, and even the scope of experience have different meanings in each ontology. These differences, formed prior to the entry of claims into the forum of public reason, suggest a slight but vital change in Hume's observation: Reason is not the slave of the passions but of ontologies that use reason as an instrument to mandate outcomes.

Such rival ontologies as Christian Science and secular medicine are difficult to reconcile or manage with any form of reasoning. Imagine the respective members of each introducing claims to the public space of the ideal forum. Collateral principles are often used to resolve disputes of this type. In this case the minor (and hence incompetent) standing of infants can be used by both communities to impose a form of treatment, on the ground that children cannot give informed consent. But what is the treatment to be? Identification of the most effective treatment differs radically as one moves across the ontologies. The state must select and enforce one of the rival ontologies, for public reason will not produce an outcome from a deliberative inspection of evidence and argument.

Imposing one ontology over another is not a call for grieving. Courts routinely select and enforce some claims against others. The logic of a forum does not guarantee that reason will find the consensual outcome that will satisfy all claimants. Also, outcomes can, and probably have to be, nonneutral in their effects. But the enforcement of one point of view in a dispute is legally and morally authoritative only if the procedures of deliberation are in some way not reducible to the languages and interpretations that identify the rival claims. The scope of the problem is indicated by the concession that even neutrality may not be renderable in neutral terms, since what it means to be neutral is itself disputable (see elaborations of neutrality in Goodin and Reeve 1990). Principles may be *regarded* as neutral, and general, as they are accepted by the citizens of a state and as they are accepted as neutral. But this pragmatic sense of neutrality, and generality, cannot be sustained in the type of pluralism at issue here; and if deliberation is dependent on partisan interpretations that precede reasoning, then even the pragmatic test is lost: Public reason is no longer grounded in an overlapping consensus but in those values that are exclusively identified with factions. The problem is that rival ontologies can dominate public reason by swallowing all the vocabularies marking off public space.

The communities housing each set of beliefs, the Christian Science religion and secular medical practice, are within the political system in the thought experiment. But an agreement to abide by the outcomes of public reasoning also contains expectations that a set of political principles will produce a decision on independent criteria of fairness. It is not evident that an independent and relevant set of principles can be found for adjudicating claims emerging from rival ontologies. What counts as evidence will differ between the two communities, and rules of inference and argument will be influenced by the competing understandings of reality.

Political liberalism is famously different from comprehensive versions of liberalism that insist on liberal thought throughout the society (Rawls 1993). A political liberal can tolerate nonliberal values so long as citizens subscribe to the principles of governance that mark off the political domain and accept the practices needed to sustain the political order. But the principles must be independent in directing or guiding those who are responsible for adjudicating disputes. Public reason depends in part on the capacity of the political domain to address and resolve community disputes from a distance that satisfies impartiality.

Impartiality is crippled by the first two types of dispute. In the case of sincere speakers, the vocabularies of governance are empty without the meanings assigned to them by the disputants. The dispute is in effect reproduced in the political domain. In the case of radical speakers, the governing language has no power to resolve the dispute. The rival beliefs can so completely absorb the vocabulary of reason that nothing is left as a *public* dialogue. Reason itself is a contentious topic in disputes among speakers of the second type. Disputes must be resolved by other arrangements (MacIntyre 1981, 1988).

Defectors

The third type of speaker produces the least complicated dispute. A person who defects from a cooperative effort presents a classic problem of compliance. Imagine now a person who is competent, understands and has accepted the authority of the political domain, and consents to the principles that govern the relations s/he has with other members of the political system. Now suppose that this individual fails to comply with legally constituted rules of the system. If appeals emerging from the exercise of public reason do not succeed in ensuring compliance, then the use of coercion is justified on rational grounds. It sometimes seems that this direct and simple type of dispute is the paradigmatic case on which liberal governance develops its concepts and working logic. Failure of any part of the conditions of this dispute—such as competence or consent—yields modifications and extensions of the liberal program. But the case is too easy in its reliance on common understandings in liberalism. The pluralism on which liberalism is predicated can also extend to the meanings and relevance of governing vocabularies in public reason, as the first two types of dispute demonstrate.

TYPES OF PLURALISM

It helps in framing and addressing these breakdowns of public reasoning to know that a social order can be pluralist in several ways. One is the dimension of scope. Sincere speakers, exemplified here by the dispute over abortion, share a considerable property in a single conceptual scheme. They may have common beliefs on a wide range of events, relationships, and theories, and they generally accept a set of democratic rules and principles. But they diverge dramatically on the meaning and scope of life. Radical speakers do not have strong overlapping views. For example, Christian Scientists differ from secular medical practitioners on foundational views of human experience. Each has a conceptual scheme that is mainly separate from the other. The scope of disagreement is comprehensive.

A second dimension is depth. One can disagree about truth, or the concept of truth. Two persons, for example, can disagree on whether the more effective treatment for meningitis is meditation or antibiotics. This is a disagreement on truth. They can also disagree on what it is that makes the two propositions true. This a disagreement on the concept of truth. The depth of a disagreement can be extended or contracted. Abortion partisans disagree on the concept of truth, but the dispute can occur even when the conceptual dispute is confined to the comparatively narrow vocabulary of abortion issues. The dispute over healing that the second type of speaker represents is a disagreement over the concept of truth that generates two exclusive and comprehensive conceptual schemes.

Still another dimension is represented by differences that occur even when individuals agree on the meaning and application of governing vocabularies. The defector produces a dispute within a common conceptual scheme. There is no disagreement here on truth, the concept of truth, or any important issues of scope. The dispute can be due to material or moral differences, or to defects of various types, including failure of will, flawed character, simple greed, miscalculation, or any of numerous conditions that lead persons to disputes within a shared conceptual scheme. But whatever the sources, these disputes are neither complex nor elusive for public reason.

The first two dimensions, however, introduce a pluralism that undermines merit forms of public reason. Dispute management is difficult when political differences are informed by these different understandings of human experience simply because each side must view the other as wrong on both method and conclusion. The pluralism that results from these differences is deep, meaning that more than one incompatible conceptual scheme represents the "same" social and perhaps physical reality. Each proponent of realist and epistemic truth, for example, rejects as a social practice that which defines the other. Yet, each philosophy can and does enter claims into the public space that represents political liberalism.

The problems described here occur in what may be called the conceptual center of the liberal program. Liberal perspectives on truth require the absence of a unique interpretation of language and experience. From this requirement follows the pluralism of belief characterizing liberal regimes. Beliefs may also differ, however, on the sense and reference of governing concepts and principles. This secondary extension of pluralism to the central areas of political authority can reduce the governing languages of the liberal state to partisan interests. The expectations of objectivity and impartiality that historically have characterized theories of the rational state are difficult to maintain if governing vocabularies can be appropriated for partisan use in politics. In yet another version of the self-reflective (e.g., liar) paradox, the liberal state originates in a pluralism that can turn on itself and in doing so compromise the authority of the state to govern differences. Stated bluntly, a state that originates in the acceptance of parochial over generalizable truths loses its distinct and independent standing if it cannot manage the extension of disputes over truth and the concept of truth to its governing languages.

MORAL BELIEFS

A number of measures have been proposed in liberal theory to stabilize authority when political pluralism affects the capacity of the state to govern. These measures usually address religious beliefs, but the general points hold for any divisive doctrine. The most direct measure is to bracket divisive beliefs and claims (discussed in Rawls 1993, 247–54, as the "exclusive view"). This proposal appears in strongest form in theories of political liberalism in large part because bracketing simultaneously ensures both effective governance in divisive conditions and separations between communities and the political domain. The liberal state, as an artifact created by member communities with rival comprehensive doctrines, must be free to some degree of community values if it is to govern. The difficulty of reconciling diverse moral beliefs in the absence of authority is the occasion for the formation of the state in liberal theory. Obviously, the full introduction of incompatible moral doctrines into the political domain re-creates the disputes and differences that the state is to manage; and doctrinal views that absorb political vocabularies also compromise the separation between state and community. Bracketing these moral views and using political principles to manage disputes is one way to avoid these problems.[5]

A second approach is developed in later work by Rawls. In the 1993 edition of *Political Liberalism*, he allows claims and appeals formed on comprehensive doctrines into public space if they strengthen public reason. The usual examples offered to elaborate this view are drawn from times of political and moral crisis, such as the abolitionist and civil rights movements (Rawls 1993, Lecture VI, especially pp. 240–54). These more inclusive versions of public reason are still stringent, however. They may require believers to shape their doctrines to accommodate political needs, and in any case they are little more than bracketing with more explicit conditions for entry. Now, in what Rawls calls the wide view of public reason, a third proposal has emerged.[6] Certain types of comprehensive views may be politically acceptable if they are consistent with liberal principles, and the political claims that follow from them are supported by the values and principles of political liberalism. These are the threshold tests found even in the wide view of public reason. They tell us that the political domain is still to function as a gatekeeper for community values and to remain as the dominant set of governing practices in society.

It can be helpful to remember certain history lessons in evaluating political theories that rely on distinctions between political and moral languages, in particular those famous exercises that instruct us on the failures of partitions between facts and values. Intimate relationships between descriptive and value statements are legion and include the occasional deductive relationship, various overlaps of descriptive and evaluative languages in a number of social practices, and the strong normative influences of social structures on what may appear to be neutral descriptive languages (Foot 1969, Searle 1969). But the predictable observation that bracketing any dimension of language is exceedingly difficult, especially when the language is on the normative order of morality, misses the opportunity to elaborate on the many nuanced relationships between moral and political domains that help craft a logic of public reason.

The close etymology of moral and political/legal languages hardly needs mentioning. Both are normative and expressed in forms of practical reason that aim to govern relations among persons and groups in human communities. Morality is usually concerned with the protection and provision of a set of prudential goods that includes life, health, integrity, and truth and that typically extends concerns for realizing these goods to others as well as the self. Both autonomy and liberty are necessary conditions in moral actions. Politics is a concept that decomposes into such terms as freedom, obligation, legitimacy, and authority. Coercion is one method for directing actions in political settings. Most of the modern vocabularies in morality and politics seem reasonably distinct from (though often dependent on) theoretical terms—such as explanation, causality, and even understanding—that lead to statements about the way the world is.

Various definitions of morality can be found in contemporary philosophy, which provides at least three plausible models of moral reasoning: deduction, induction, and coherence. These models are more or less useful in different settings, though even a cursory reading of this paper will display the affinities of the arguments here for a coherence model (also favored by Rawls). The relevant point is that both political and moral reasoning are represented by these three models, and there is no reason to think that the formal expressions of reasoning can demarcate morality and politics.[7] It is also improbable that the state can be insulated from the peculiar form of morality Rawls (1996, 13, 175) labels a comprehensive doctrine or that such doctrines are hostile to the political deliberations of public reasoning.

The state is clearly a kind of moral undertaking (Rawls 1993). But its distinctive moral framework is also influenced by the moral beliefs and practices of its constituent communities, which are among the main resources for defining political issues and resolutions. Even the strongest advocates for a separation between political and moral reasoning acknowledge the type of broad influence represented by the abolitionist and civil rights movements in the United States. These movements, inspired by the morality of religious beliefs, were critical in expanding the criteria for membership in the political system. More generally, moral beliefs can affect modern political societies by defining and redefining the languages of politics. Principles are given a different scope, perhaps also a different sense, as the result of moral influences. Morality can also reshape the dimensions of public space, changing the standards for political effectiveness and the rules of inclusion and exclusion for issues. These moral effects are not just found in times of crisis. They are perennial features of political life. Many originate in beliefs comprehensive enough to raise the suspicions of a political liberal, but still they seem to have beneficent effects in restructuring the scope and logic of public reasoning.

One explanation for the congenial associations of comprehensive doctrines and public reasoning is the obvious distinction between comprehensiveness and divisiveness. Some doctrines are not only comprehensive but also inclusive and even conciliatory. Buddhism is an example. Contrast this encompassing philosophy with current views on abortion, many of which are only partial cuts into human experience but exceedingly harsh toward opponents. That doctrines are comprehensive, meaning that they provide a complete articulation of all values and virtues for the moral life, is not a relevant consideration. Nor is the observation that a doctrine is a form of community morality. Divisiveness is the relevant consideration in public reasoning, and it is also the main issue in the conditions of pluralism described here.

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[5] Rawls 1993, Lecture VI, especially pp. 240–54. If the moral values are unavoidably divisive, they must be suspended for the political process to work successfully. This requirement is nothing more than a restatement of the initial agreement in any version of the social contract to be bound by the principles and outcomes of the political domain.

[6] Rawls elaborated this view in symposiums at the University of Michigan (March 1995) and the Harvard Divinity School, the latter published as Rawls, Sandal, and West 1995.

[7] Since I am denying sharp distinctions between moral and political reasoning, I do not provide a full definition of either morality or politics. For an unusually clear summary of these three models of moral reasoning, see Beauchamp and Childress 1994, chapter 1. Any number of texts and anthologies in moral philosophy elaborate the relationships of truth to moral thinking, including the standard books by Hudson (1970) and Castaneda and Nakhnikian (1965). Also see Frohock 1978 for a discussion of the concept of politics.

The effects of moral reasoning on political languages are sometimes overriding. Consider, again, the thought experiments on abortion issues and medical therapy. These disputes close the separation between moral and political languages that liberalism establishes. Political terms in these disputes do not have an independent sense and reference. Their meanings originate in the communities that form the political domain and are relevant as they bear on political issues and problems. The abortion and therapy disputes are about the protection of life and the right to exercise discretionary authority over one's body and the health of one's children. It is hard to see where the moral and political can be demarcated as these matters enter public space. Bracketing moral beliefs in these cases seems to remove the political dimensions also. Holding divisive moral disputes out of the political machinery is a peculiar suggestion on any grounds. The upshot of the proposal is that the state is proportionately reluctant to adjudicate issues as they become more obvious candidates for adjudication. This seems to be the inverse of the proper ordering of state action and political need.

Even those arguments that establish political dominance over morality do not break all ties between the two practices. The most direct form of political dominance is state adjudication of intractable moral disputes. The abortion dispute cannot be resolved by the disputants. It must be managed by the state, which legitimately does so on the basis of the needs of the political domain. But it is fatuous to think that any resolution, even one that temporizes by maintaining the status quo, can be independent of the moral views of the rival doctrines. Any imposed or negotiated solution will represent portions, perhaps wholes, of the prolife and prochoice positions. The same observation can be made of the therapy dispute example. Shielding Christian Science from regulation implicitly endorses a meditative approach to healing, or at least the political value of tolerance over the maintenance of life for children. Invading the religious community with imposed medical therapy amounts to a rejection of prayer as a form of healing. In any construction of the state's role there is complicity in a moral point of view, typically (though not necessarily) including a moral belief drawn from one of the disputing communities. Even a political dominance of morality justified by extreme conditions, say, the survival of the society, is still dependent on moral views. In this case survival would have to be morally weighed against the values that would be sacrificed by maintaining the political community. Not all social orders may be worth maintaining from a moral point of view (Hart 1963).

The intimacies of moral and political reasoning are nowhere so explicit as in the plea for reasonableness that characterizes liberal regimes. The threshold test for membership in liberal political systems is a willingness to accept the reality of opposing views and the mutual advantages of a system of adjudication for the disputes that follow pluralism. The practical result of the acceptance is that individuals subscribe to a social order in which all moral doctrines must be minimally governed to secure the general advantages of social cooperation. That may require the denial or contraction of any particular moral claims. An individual who is not reasonable in this minimal sense obviously is not a member of a liberal political system. This condition is found in all contractual agreements.

But what it means to be reasonable is still complex. Both slavery and abortion are disputes in which there is asymmetry at basic levels. Unlike a disagreement over the role of the state in the economy that rests on common economic frameworks and variables, these two disputes are traceable in substantial part to radically different definitions of the human person. Whether one can be reasonable is a function of these deep considerations. If one believes that slavery and abortion are (respectively) denials of humanity and murder, reasonableness is not an opportunity, even if one accepts the advantages of general adjudication. In the phrase of the day, moral reasoning will occasionally require zero tolerance for certain actions and practices by compelling a political view. In the "Evangelium Vitae" (John Paul II, 1995), for example, the inviolability of human life assigned to the first hours after conception requires a political opposition to legalized abortion. Put more generally, morality affects politics by requiring that allegiances to the state be always conditional, never absolute. The practical effect is that whether one can be reasonable on any particular set of political issues and problems is influenced by moral values. Also, the deep pluralism expected in liberal conditions can be expected to produce rival understandings of reality and moral principles. This extended pluralism will give different interpretations or weights to the sense of reasonableness in different parts of the political domain.

Public reason may be more accurately understood not as a check on morals at the entry points to politics but as a device to introduce morality into political domains. The language of the forum is sometimes decisively invested with community values, and these languages cannot be generalized among communities if the conceptual distance among them is too great. The state is a moral player in closing these spatial differences among community languages and in introducing moral perspectives to political needs. Public space may then be seen as a negotiated space that varies with moral and political issues and with types of speakers. The authoritative role for public reason may be as a kind of filter and monitor of generic moralities as they enter political deliberations and of political domains shaped in part by the influence of moral reasoning.

PUBLIC REASONING: FROM MERIT TO COLLECTIVE CONSIDERATIONS

Theories of public reason that bracket divisive views are continuous with the basic liberal impulse to partition human experience. The demarcations among state, society, and individual that mark the historical transitions to classical liberalism establish the grounds for concepts of public space and impartial deliberations removed from divisive influences. But if the insulation between state and community is porous, then an adequate theory of public reason must accommodate the extension of pluralism to governing languages without sliding into political chaos or moral nihilism.

The arguments here indicate that fortifying the partitions between state and community with a more refined political language is a gatekeeper exercise in futility. Nor can it be worthwhile to thicken the liberal program with additional values or interpretations of extant principles as a condition for playing the liberal game. Some recent contributions have padded the liberal program in remarkably strong ways. The liberal state is now regarded by many distinguished theorists as a community with overriding values and goals that have content (they include the good as well as the right) and must be accepted by citizens if they are to be members of the liberal political system.[8] But if language is open to rival meanings, then thicker brackets and additional layers of values and interpretations will also be proper targets for redefinition and partisan use. Language does not escape its elastic foundations by making terms more explicit or elaborate or by adding normative content (like virtues). That exercise is roughly equivalent to increasing the volume of one's voice in talking to those who do not understand one's language. Shouting does not work.

A more effective approach to public reasoning than bracketing divisive beliefs or imposing gateway interpretations on political vocabularies is to recognize exactly what *form* of reasoning can be maintained when governing languages yield multiple interpretations. Two complementary approaches suggest a workable theory of public reasoning. One is the rejection of all forms of computational reasoning. At its formal extreme, computational thinking is top-down reasoning, meaning that first-order rules and principles are applied to particular cases in step-by-step procedures. This serial type of reasoning resembles a Turing (1950) machine as it decides the fit between general and particular levels of a system within the provinces of the system itself.

A computational system has a finite and listed set of transition rules which, when applied to any state of the system, provide for the deduction of a subsequent state. The requirements for such a system are easy to specify: The system is closed in the sense that all the variables affecting it are abstract components of state descriptions and the transition rules, which means that the formal structures of the system are its states and the set of dependency relations found in the transition rules. No other exogenous variables intervene. Certain physical systems, perhaps only in a laboratory setting, can meet these requirements. A controlled experiment that limits variables to their causal laws and sequential states represents computational reasoning. John Searle's Chinese room experiment qualifies as an imaginative computational system, if the simulator managing inputs and outputs exhausts the mechanisms of the system *and* the system lacks consciousness.[9]

A local and homogeneous political community, one in which the primary values and transition rules are understood without qualifications, may be able to use algorithms to produce subsequent states from previous states. But we are reminded of Hart's (1961, 89–96) caveats on systems of law without rules of recognition: They cannot address unanticipated cases or disputes over the scope and meaning of primary rules. These limitations are flaws that make computational systems unlikely models for public reasoning. That authorities typically cannot actually compute in mechanistic fashion is one of the points in the thought experiments elaborated here. Contemporary theories of language also concede the limitations and perhaps impossibility of computational reasoning. It may be that computation devolves to coercion to shield political structures from the natural inquiries of citizens. But the point is that the stability and even possibilities of computational political systems depend on an acceptance of the properties of the system either spontaneously or coercively maintained. Unlike physical systems, a human community must introduce dissenting powers in order to complete the range of variables producing the computations.

In the strong pluralism introduced by radical speakers, noncomputational reasoning must dominate, on the assumption that a reduction of governing languages to partisan disputes leaves all precepts open. An unrestricted and continuing dialogue is a version of noncomputational reasoning that suffices in public domains. When successful, it is a group form of parallel processing: multiple and simultaneous explorations of proposals with many kinds of techniques. Listen to a good conversation. Narratives and linear modes of reasoning compete and complement one another in the movement of expressed thoughts. The goal of the conversation is mutual understanding of alternatives and resolutions and, finally, an acknowledgment of the best or just outcome. The logic is bottom-up in the sense that first-order rules and principles do dominate but only guide. Procedures (at best) are in place to yield a solution that is not specified in advance. The system of reasoning is creative, not algorithmic. Persons who reason in this system must be open to unanticipated possibilities. Computational reasoning is to be controlled by an acknowledged text. Imperatives and prescriptions for action follow (within a narrow range) the acceptance of acknowledged rules and principles. Noncomputational reasoning can have no controlling text. The text (if there is one) is produced from deliberations as a crafted and open instrument of dialogue. The familiar model of an *open* conversation is the natural form of public reasoning when governing languages are subject to rival and reasonable interpretations.

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[8] This "tough love" version of liberalism is developed (in different ways) by, among others, Macedo 1990, Galston 1991, and Rawls 1993, and it provides neat and quick solutions to many problems widely viewed as dilemmas for liberal theories. See, for example, Rawls's (1993, 199–200) easy judgment that, of course, dissident resistance to standard educational curricula must give way to the needs of democratic citizens for a secular education. All the agonized reflections in, for example, *Wisconsin* v. *Yoder* (1972) are presumably just misunderstandings of the thickness of the liberal program and the scope of the political domain in ensuring the skills needed for public reason.

[9] Of course, some of Searle's major critics do assign consciousness, or at least understanding (Dennett 1991, 435–40), to the system of inputs and outputs. For the original description of the thought experiment, see Searle 1980.

The self-reflective powers of a conversation collapse venerable distinctions between political and philosophical dialogue (see Barber 1988 for the distinctions). The defining style of political talk is to take anything relevant to the issues at hand into the discussion. A lively political argument on abortion, for example, will easily move among and between distinctions in species of dialogue almost from moment to moment, including those between theoretical and argumentative talk. Discussions on the proper regulation of abortion are impossible without references to the origin and meaning of life, the standing of the fetus (or unborn child), a woman's right to control her own body, and, in general, the sense and scope of prochoice and prolife languages. But all the distinctions are open to movement, including that between speaker and hearer (which typically can barely be recognized in the heat *and* light of good conversation). A noncomputational version of public reason is dialectical, and it has an unremitting scrutiny that makes all communication transitional and provisional and all items (including organizational frameworks) part of the material for discussion.

A second approach to public reasoning identifies the one defining constraint that avoids the risk of chaos and nihilism suggested by unrestricted interpretation: a concern for the collective arrangements of the political society. Collective references are easily found today throughout public reasoning. They lead us away from merit forms of public reasoning to those larger considerations that have always been a part of theories of the state. In *Wisconsin* v. *Yoder* (1972), a case that has become a critical test for rival understandings of the scope of the liberal state, the Supreme Court addressed the issue of balancing the state's interests in universal education against the free exercise clause of the First Amendment and the interests of parents in rearing their children. The Amish argued that enforcement of the compulsory attendance law after the eighth grade would endanger, if not destroy, their religious beliefs and hence their community. The state argued that the critical skills taught in secular public schools are needed for citizenship in a liberal democracy and to survive economically in the modern world (Choper 1982).

Amish practices represent a separate way of life. The abandonment of violence and most accoutrements of modern life are tokens for an independent cultural existence in distinct geographic regions. But the Amish are also members of the American liberal political system. They presented themselves in court during *Yoder* through their attorneys, argued their case according to rules of evidence and argument stipulated by the legal system, and indicated that they were willing to abide by the Court's decision. The gravity of the issue for them is dramatized by data indicating that their young may be increasingly inclined to drift away into Western culture (Erickson, Erickson, and Hostetler 1980; Hostetler 1993; Kraybill 1989). But even more important, the meaning of Amish culture is fixed in part from its existence in Western societies. The Amish, as an insular community opposed to many Western values and beliefs, are premodern in their resistance to such technologies as television and the telephone. None of these observations could be made if the Amish were a community geographically isolated from Western culture.

A similar context-dependent definition of Christian Science is persuasive. If the core belief of Christian Science were expressed in a theistic society that had little contact with secular views on medicine, especially if the dominant and perhaps exclusive religion were Christian Science, the meanings of the defining religious statements would be different. The current expression of the belief that illness is an illusion with no ontological standing is semantically linked to the existence of modern medicine. The concepts of disease, the body, and health on which the claims of Christian Science are developed rely on the vocabularies of medical practices. The disease that is illusory, for example, is not a version of spiritual possession prominent in Scripture but is the empirical sense of disease found in current medical theory and practice. Understanding the full import of Christian Science also requires that one be conversant with recent medical practices. Even the radical standing of Christian Science views on illness and health depend on medical advances. Imagine, for example, how prosaic these views might have been at a time prior to the impressive advances of modern medicine. Community beliefs, even as tokens of moral realism, depend on conventions for their full meanings.

Semantics are not the full story. The effects of arguments drawn from beliefs vary with context. To see this pattern of influence, look at two of the opposing forms of argument found in disputes like *Yoder*. Let the presentations drawn from religious beliefs be called *exclusive*, meaning that the arguments move in one way to one truth that is regarded as generalizable to all reasoning persons. Those who make such arguments can express their identity in the argument and the truth that the argument presents. Let presentations from critical perspectives be called *inclusive*. These entertain a menu of possible arguments leading to similar conclusions and the possibility of multiple truths from different arguments. The person making the argument and drawing the conclusion may remain distant from method or outcome in maintaining an impartial or objective stance. In this type of presentation the person is invited to develop argumentative skills that provide entrée without commitment to various truths and concepts of truth. The antagonism between these forms of arguments is undeniable. If one maintains, for example, that only an uncritical immersion in religion will provide for religious experiences, then the critical thinking of liberal institutions of learning (which require a distance from subject matter that avoids commitment) would be hostile to one's religious needs. Similarly, the invitation to immerse oneself in a belief would be repugnant to anyone who is merely surveying religious experiences from a detached point of view.

Now let a political society be homogeneous or heterogeneous with respect to these two types of presentation. Society A has a membership consisting only of religious persons (who, for the convenience of this thought experiment, have common arguments and truths). Society B has only supporters of critical perspectives as its members. A third, C, is mixed—both believers and critics (let these terms represent religious and critical perspectives) are found in the political domain. The standing of each type of presentation will vary across these societies. In-house presentations, such as those from believers in A, will be reinforcing, while external presentations, a believer's presentation in B, for example, will be overriding or undermining. The value of presentations also varies among the societies. Critics may find their presentations more important when they visit A, less important in B. Believers may present a mirror image of this evaluation, assigning a higher standing to proselytizing among the critics in B than reinforcing the faith in A. But whatever the particular assignments, in both A and B the presentations of each perspective will have different functions and values.

Society C is more complex, with a subtle range of functions and values. In this society, which represents more adequately the setting for the liberal state, state exemptions and regulations turn on proportions and equilibria between exclusive and inclusive presentations. In *Yoder* the Court examined the scope and location of the Amish in the political system. The fact that the Amish are a relatively small and passive community with little capacity to disrupt or alter secular values probably was a factor in the decision to grant an exemption for their educational needs. If a fundamentalist sect of impressive size had petitioned the Court similarly, then the same utilitarian considerations would have provided the justices with considerations against granting an exemption.

A form of public reason that accommodates a more complete range of human experience could provide for both exclusive and inclusive presentations in social arrangements. Pluralism, remember, can be satisfied along a number of dimensions. One is B, a society of those who are uncommitted to a single overriding truth and prepared to tolerate a diverse set of community beliefs and ways of life. This type is the liberal society through and through, in which pluralism is secured through epistemology. We might label this intellectual pluralism. Another type of pluralism is found in the social arrangements of C, which can also include dissimilar moral communities. This might be seen as institutional pluralism. Now, if both exclusive and inclusive presentations, religious beliefs and critical perspectives, are devices widely used to render human experience intelligible, a judgment exceeding the simple merits of cases can select the second type of pluralism on the grounds that it is more robust in meeting human needs. Decisions on exemptions or regulations might be guided accordingly. The arrangements of communities—their relationships to one another, the set of beliefs and claims emerging from them, their relative size and distribution, the political implications of beliefs—are the primary considerations in this type of public reasoning. They enter public reason as holistic considerations and as semantic contexts for the presentations that each philosophy makes.

It is an axiom of liberal realities that some communities in the liberal state do not meet the standards of liberalism. Churches and the military are two prominent examples of nonliberal institutions which may flourish in liberal settings. The standing issue in liberal political societies is the extent to which liberal values, especially autonomy, are to be protected by the state. A shift to collective considerations allows the state to permit nonliberal practices if the arrangements of the larger society are enriched or not threatened by these arrangements and the practices pass minimal thresholds identified by moral reasoning. For example, a liberal state may allow gender-based hierarchies in consensual religious communities but not a coercive exploitation of one sex by another. The logic of collective reasoning allows the acceptance of values that one would not generalize to the entire society if these values are conducive to the productive flourishing of diverse human communities. A state governed by such considerations is charged with the protection of diversity rather than the enhancement of liberal values (Galston 1996).

I have argued that the critical thinking represented by noncomputational reasoning must dominate public reasoning in conditions of strong pluralism. But the liberal state that relies on collective considerations can still favor a distribution of beliefs and communities dense with closed forms of organization. In this sense the arguments here both align with and dissent from liberal critics who reject "the universalist pretensions of liberal theory" (Gray 1996, 360). The open dialogues that represent liberal styles of thinking and talking are privileged as modes of public reasoning but not in the larger society of communities that constitute the liberal state.

Unlike every other community in the political society, the state must attend to the complete arrangements of collective life. Public reason crafted on this understanding moves beyond the merits of cases to more general considerations. This expansive sense of public reason will not be contained within a juridical form of reasoning that shields its deliberations from external considerations, in this instance the needs of the political society. One consequence will be a loss of the impartial face that models liberal expectations. But, then, it is the vain effort to secure impartiality by attending exclusively to merit that paralyzes public reason in those divisive conditions when it is most needed. Collective terms may be the first anchor in a version of public reason that succeeds in considering both moral and political arguments.

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A Not-so-distant Mirror: the 17th Amendment and Congressional Change

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At the beginning of the century, the Constitution was amended to permit direct election of U.S. senators. We examine the shift to determine the extent to which an electoral reform can result in meaningful change. Variables are analyzed that tap the Senate's membership and responsiveness before and after direct election, and House data are employed to control for history effects. The results indicate that changing the mode of senatorial selection did indeed lead to alterations in the composition and sensitivity of the Senate, a finding that should encourage caution regarding the electoral reforms being advocated at the end of the century.

The selection of U.S. senators by state legislators occurred for the last time in November 1912. Just a few months later, on May 31, 1913, the 17th Amendment to the U.S. Constitution went into effect. It brought down the curtain on indirect elections by altering Article 1, Section 3, such that subsequent Senate vacancies were filled via direct popular elections. Indirect senatorial elections had always been controversial. At the Constitutional Convention the procedure eventually passed 10–0 (Madison [1840] 1987, 87), but Alexander Hamilton, Elbridge Gerry, James Madison, and especially James Wilson all expressed reservations. Wilson's advocacy of direct elections was taken up by others over the decades and began to gain great momentum in the late 1880s (see Swift 1996, 176). In the first session of the 52d Congress (1891) alone, the House received seven memorials from state legislatures, 54 petitions from interest groups, and 25 proposed resolutions from members of Congress advocating direct popular election of senators (Riker 1955, 467). In fact, in each session of Congress between 1893 and 1911, the House passed a popular election amendment only to see it fail in the other chamber, where sitting senators had a vested interest in preserving a method of selection that had been good to them (Perrin 1910).

Impatience with this situation led a number of states, beginning in 1904, to adopt the "Oregon model," which extracted pledges from state legislators that they would honor the outcome of popular referenda on the Senate contests in their state. Passage of the constitutional amendment should thus be seen as the culmination of a movement that brought about what Senator Robert Byrd (D–WV) believes to be "the single most important reform of the Senate" (1988, 387). Alan Grimes goes farther: "No amendment has so fundamentally altered the design of the original structure of government" (1978, 2). Not surprisingly, observers expected this reform to have momentous consequences. Prior to enactment, both supporters and opponents were issuing incautious predictions. Perhaps the most egregious came on the Senate floor itself, where Senator George F. Hoar (R–MA) intoned: "Let no man deceive himself into the belief that if this change be made, the Senate of the United States will long endure" (Congressional Record 106 [1893]). Others, such as Senator Porter McCumber (R–ND), claimed direct election would result in the complete elimination of political parties (Congressional Record 47 [1911]). Proponents were equally hyperbolic. They believed direct election would eradicate political graft, bribery, and corruption, thereby revitalizing the entire political system by shifting power from parties and state legislatures to "uncorrupted" lay voters (Daynes 1971, 15–6; Lowry 1911; Mitchell 1896). As George Haynes (1938, 1005) later noted, "the champions of the change predicted that it would effect revolutionary reform in the personnel of the Senate."

Somewhat surprisingly, systematic analyses of the actual as opposed to expected consequence of direct elections are not plentiful. King and Ellis (1996, 72) observe that, "oddly, there have been few academic studies of this amendment, and fewer still that have employed the theoretical insights and methodological sophistication of modern social science" (but see Crook 1992, Daynes 1971, Ellis and King 1996). To the extent an opinion has been formed, it seems to be that the consequences were minimal. Haynes, once a leading advocate (1906), was disappointed with the reality of direct elections, writing that no large effect was evident (1938, 1005). Bickel (1968, 3) sees the institution of direct senatorial elections as perhaps the quintessential example of the "illusion-disillusion" pattern of democratic reform. Daynes agrees (1971, vi), and he concludes that expectations were "exaggerated and overstated" (1971, 149). But for the most part such conclusions are derived from impressionistic rather than systematic analyses. We believe a more concerted investigation is merited.

Much can be learned from determining the extent to which changing the method of senatorial selection

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altered the Senate itself. The objectives of reform were unusually clear and afford us the opportunity to ascertain whether they were accomplished. Are Bickel and others correct to be cynical about the prospects for democratic reform, or is their conclusion attributable to inadequate research designs? This question is particularly poignant in light of the resurgent interest in democratic reform. Just as the 20th century began with substantial interest in populist electoral reform, so is it ending. Limiting the number of legislative terms is now the reform of choice, favored by approximately three-fourths of the American population. As was the case with direct election of senators, the target is a congressional membership thought to be under the thumb of special interests and, therefore, out of touch with ordinary Americans. Many believe that changing the nature of elections will restore to the people their rightful role in legislative government. The parallels between the direct election and term limit movements, right down to the efforts of each to proceed without a constitutional amendment although one would seem to be required, are at times uncanny. The Oregon model of listing on the ballot the candidates pledged to vote in accord with the people's senatorial preferences has merely been replaced by the U.S. Term Limits model, which lists on the ballot whether a congressional candidate is pledged to support term limits. As such, using the direct election movement as a mirror may provide information on the potential effect of current electoral reform proposals.

Our primary purpose is to present systematic evidence on the degree to which the 17th Amendment had any consequences. It is impossible to test each and every alleged consequence, so we concentrate on a manageable number of what seem to be the more central and likely effects. These primarily relate to Senate composition and responsiveness. This is appropriate, since changes in the membership and "connectedness" of the Senate were the main desires of reformers.

CHANGES IN MEMBERSHIP TRAITS

We begin with the membership of the Senate. Unlike most institutions, democratic legislatures are often evaluated by membership traits. After all, as Hanna Pitkin (1967) and others have so ably argued, descriptive as well as substantive representation is important. To many people, it is essential that a legislative body *look* a certain way, not just *act* a certain way; besides, it is commonly felt that the former affects the latter.

Such was the thinking, at least, among those who argued for changing the mechanism by which senators are chosen. Supporters of direct elections were convinced that reform would alter the kind of person typically serving in the Senate, and they wanted desperately to accomplish this goal. Reformers felt that senators were aristocratic and detached from ordinary people, that they came from aloof and monied families, and that they earned their Senate positions not because of demonstrated expertise in affairs of government but because of family connections (Agar 1950, 575–82; Phillips 1906; Rothman 1966, 250).

Of course, where proponents of reform saw the need to populate the Senate with ordinary people of the state, defenders of the status quo feared that direct election would open the doors to "a crowd of incompetent, unfit, rash, socialistic and radical men who have no proper views of government" (exchange between Heyburn [R–ID] and Cummins [R–IA] on the Senate floor, Congressional Record 47 [1911]). But whether a more popularly oriented body was viewed as improving or harming the polity, the point is that both sides expected the switch to have a significant effect on the composition of the Senate. Is there evidence to support this belief?

It is difficult to acquire objective data on the degree to which a senator is aristocratic and under the thumb of large monied interests, such as the railroads, or is a reasonably typical citizen of the relevant state. We employ two measures that are suggestive but not definitive. The first is whether a senator's family was a wealthy political dynasty. The second is the extent to which a senator had previous experience in government.

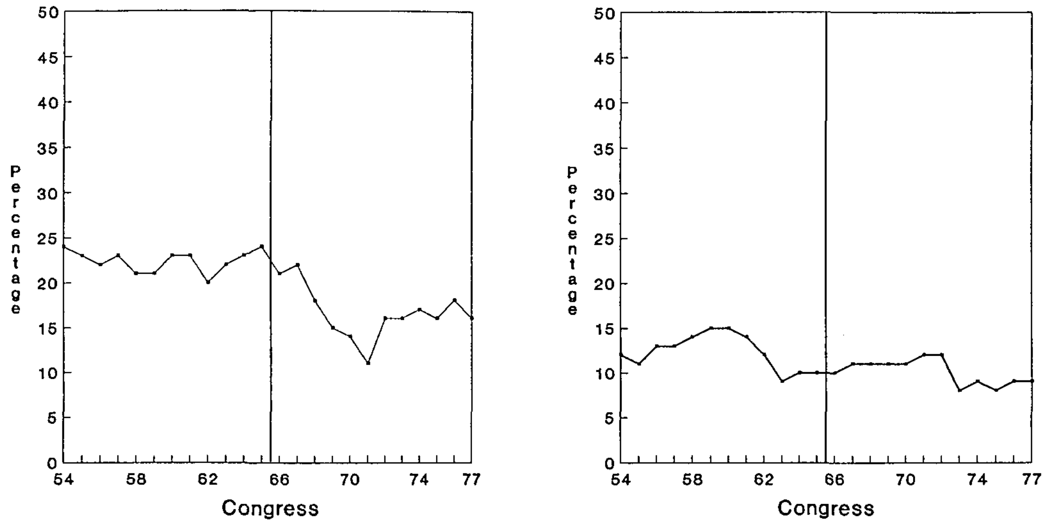
POLITICAL DYNASTIES

Critics of indirect election believed the process afforded special advantages to the well-connected and the well-heeled. It was argued that direct election would demonstrate that ordinary people were not so impressed with family dynasties and the trappings of wealth. One partial test of this expectation is whether a senator had a relative who previously served in Congress. The hypothesis is that direct election will diminish the percentage of senators from politically influential families. Individual-level information on senators with such relatives was aggregated for each Congress so that changes over time could be analyzed.

A danger in this type of analysis is that any change observed may be attributed to the advent of direct election but may be due to broader historical forces. On this count, bicameralism proves to be analytically useful. The enactment of direct election to the Senate should be largely irrelevant to the composition of the House of Representatives. We have data on the percentage of House members from politically influential families. Support for our hypothesis will be heightened if a similar degree of decrease did *not* occur in the House, where election mechanisms were not altered.

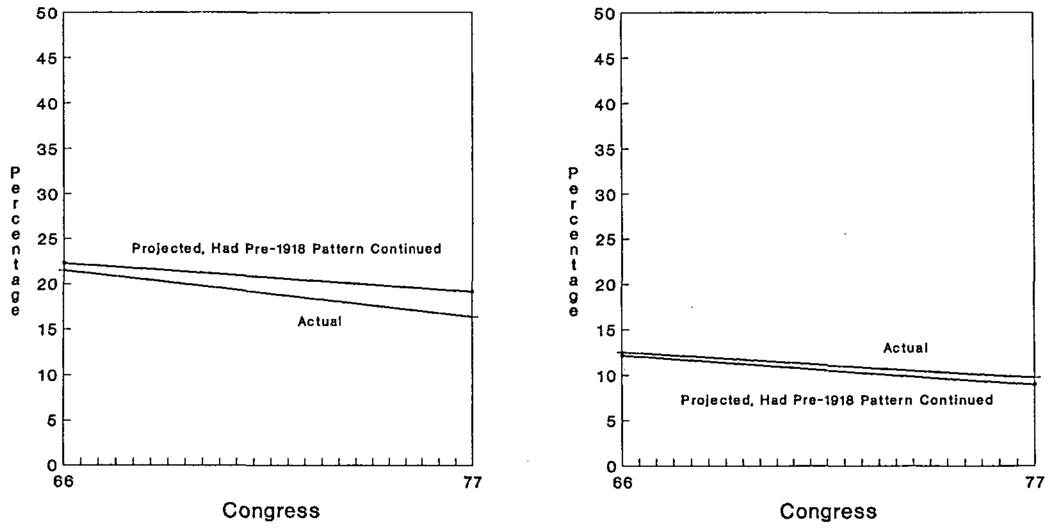
In Figure 1, we juxtapose the trend in the Senate (Panel A) and in the House (Panel B) for the 12 congresses before and the 12 congresses after full implementation of the 17th Amendment. As might be expected, representatives were much less likely than senators to have relatives who previously served in Congress. For the 24 congresses (54th through 77th, 1895–1942) in question, both chambers experienced a decrease, but it appears more pronounced in the Senate after direct election than might have been expected had pre-1918 trends continued. During the first six congresses after the 17th Amendment became applicable to all Senate terms (i.e., beginning in 1918),

FIGURE 1. Percentage of Members with Relatives in Previous Congresses



Panel A
Senate Data

Panel B
House Data



Panel C
Senate Trend Lines

Panel D
House Trend Lines

Source: Cleaned version of ICPSR 7803, *Biographical Directory of the U.S. Congress*, 1789–1989. See unnumbered note on page 845.
Note: In Panel C the "actual" line represents the pattern produced by direct election while the "projected" line is an estimate of the likely pattern had indirect election continued. The vertical line in panels A and B represents the advent of universal direct election in the Senate.

a marked downward trend is noticeable in the Senate; then it levels off, and there is even a mild reversal. On the House side, if anything, the decline slows after 1918, but there certainly is no sharp drop.

We regressed the Senate "political dynasty" variable on a simple counter variable (ranging from 1 in the 54th Congress to 24 in the 77th), on a dummy variable (0 for before and 1 for after 1918), and on an interactive variable (dummy*counter) in standard form (see Gujarati 1995, 512–4). This formulation is graphed in Panel C and indicates whether the intervention (in this case, full implementation of the 17th Amendment) precipitated a change in intercept (captured by the dummy) and/or a change in slope (captured by the interactive term). Even though the House did not experience change in its basic electoral structure, parallel procedures were employed for the House data (see Panel D) to determine whether larger historical trends could be responsible for changes in the Senate.

Theory leads us to anticipate that the expected decrease in the percentage who come from connected families will not occur all at once but over time, as voters become accustomed to direct election and as prospective candidates determine the type of individual preferred by the people. Thus, we expect a (negative) change in slope in the Senate (and not in the House) more than any change in intercept. OLS estimates indicated autocorrelation, so the coefficients presented below were obtained using Cochrane-Orcutt correction procedures (SHAZAM, Version 7.0).

$$Y_{SF} = \underset{(2.41)}{25.41} + \underset{(2.22)}{1.26(D)} - \underset{(.22)}{.26(C)} - \underset{(.12)}{.17(D^*C)} + e,$$

$$\text{Adj. } R^2 = .73,\ N = 24,$$

$$\text{rho} = .53 \text{ (reached equilibrium in five iterations);}$$

$$Y_{HF} = \underset{(1.66)}{15.38} + \underset{(1.53)}{.04(D)} - \underset{(.15)}{.27(C)} + \underset{(.40)}{.03(D^*C)} + e,$$

$$\text{Adj. } R^2 = .58,\ N = 24,$$

$$\text{rho} = .52 \text{ (reached equilibrium in four iterations),}$$

where

Y_{SF} = % of senators with relatives who served in Congress,
Y_{HF} = % of representatives with relatives who served in Congress,
D = dummy (0, 54th–65th congresses; 1, 66th–77th),
C = counter (1–24 congresses),
e = error term, and
( ) = standard error.

The results are somewhat consistent with expectations. In panels A and B of Figure 1 it appears as though the movement away from having relatives who served in Congress is more pronounced in the Senate and after direct elections. Fitting regression lines to the data points supports this perception. The "counter" trends (representing the pre-1918 pattern) are virtually identical in the Senate and House, but the coefficients for the interactive term (representing the change in slope occurring around 1918) are quite different. The House trend actually flattens out slightly, and the Senate trend declines more sharply than ever. This is seen graphically in panels C and D of Figure 1, where estimates based on the patterns before and after the advent of direct elections in the Senate are presented for each chamber). No doubt the Senate interactive coefficient would have been even more strongly negative but for the slight upturn in the dependent variable in the later years of the time series. The only real disappointment is that the coefficient for the interactive term in the Senate fails to meet standard tests of statistical significance—but just barely. Of course, interactive terms tend to be highly colinear with their parent variables. While not biasing the coefficients themselves, multicolinearity does inflate the standard errors, meaning variables may look statistically insignificant when in actuality they are significant.

It appears that direct election helped reduce the number of senators coming from a political dynasty. Did it also decrease the number coming from wealthy families? Not all politically connected families are necessarily wealthy, so brief attention to this trait is appropriate, particularly since a major goal of reformers was to disconnect the Senate from engines of wealth. As Haynes (1906, 173) states: "It is to be expected that under popular election there would be sent to the Senate fewer merely rich men—men whose entire past has been devoted to wealth-getting or wealth spending, and who have given no hint of any aspirations or aptitudes for statesmanship." Did direct elections have the intended effect?

The data are not available to answer this question systematically. Neither income tax returns nor financial disclosure requirements existed before the 17th Amendment was enacted. There is impressionistic evidence, however, of changes in the wealth contours of the Senate. Shortly after the turn of the century, 23 of the 90 senators were acknowledged to be "citizens of great wealth" (*Financial Red Book of America* 1903; *World Almanac* 1902). We followed the elections to these 23 seats to see whether they continued to be filled by people of great wealth.

Five of the 23 indirectly elected wealthy senators survived the first direct election in their state: Henry Cabot Lodge (R–MA), Thomas S. Martin (D–VA), Francis G. Newlands (D–NV), Reed Smoot (R–UT), and Francis E. Warren (R–WY); a sixth, Stephen B. Elkins (R–WV), was replaced by his son Davis. No movement toward people of ordinary means occurred in these cases. The history of the 17 other seats, however, suggests a modest push toward a less wealthy Senate. Judging from the *Biographical Directory of the American Congress, 1789–1989*, as well as newspaper accounts, nine of the wealthy senators were replaced by wealthy men.[1]

In other words, of the 23 Senate seats held by truly wealthy individuals before direct election, 15 were held by wealthy senators after the shift. Whether this is a victory for reformers is open to question. Clearly, the 17th Amendment did not render it impossible for the well-to-do to secure membership in the Senate (see also Hoebeke 1995, 190). Indeed, wealthy senators are much in evidence today and are not in any way incompatible with popular elections. Yet, rough estimates suggest that the immediate effect of the change was to drop the ratio of wealthy senators from about one in four to one in six, and perhaps a shift of this magnitude was as much as the reformers could expect.

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[1] Though the method is unscientific, it is actually fairly easy to identify senators of wealth by perusing these sources. Entries that mention private tutors; preparatory schools; European travel; "interests" in timber, mining, transportation, banking, real estate, and so forth; the presidency or chairmanship of firms; service on boards of directors; and the like are in contrast to entries noting public schools, involvement with manual trades, school teaching, street-level journalism, or general law practice.

GOVERNMENT EXPERIENCE

Just as elections by ordinary people are likely to decrease the number of senators from wealthy and/or politically influential families, it can be hypothesized that they are likely to increase the number with government experience. The rationale is that a solid record of government service will attract voters (more than it attracted state legislators) and eventually become a more common trait of elected senators.

The procedures used to test this expectation parallel those described with regard to political dynasty (Figure 1). The only difference is that in this case the dependent variable measures a senator's previous government experience.[2] The results are presented in Figure 2 and in the following equations.

$$\begin{array}{l} Y_{SG} = \underset{(.03)}{1.32} - \underset{(.08)}{.39(D)} - \underset{(.004)}{.015(C)} + \underset{(.006)}{.03(D^*C)} + e, \\ \text{Adj. } R^2 = .71,\ N = 24, \\ \text{rho} = .37 \text{ (reached equilibrium in six iterations);} \end{array}$$

$$\begin{array}{l} Y_{HG} = \underset{(.01)}{1.10} + \underset{(.03)}{.29(D)} + \underset{(.001)}{.007(C)} - \underset{(.002)}{.02(D^*C)} + e, \\ \text{Adj. } R^2 = .80,\ N = 24,\ \text{rho} = .00, \end{array}$$

where

Y_{SG} = mean level of senator's previous government experience,
Y_{HG} = mean level of representative's previous government experience,
D = dummy (0, 54th–65th congresses; 1, 66th–77th),
C = counter (1–24 congresses),
e = error term, and
$(\ )$ = standard error.

Previous government experience was trending downward for the Senate very early in the century, but the pattern reversed dramatically in the 1920s. The extent to which direct elections are responsible is open to question. The case would be stronger if the increase had commenced closer to full implementation in 1918 (see also Daynes 1971, 100). Still, members of the House certainly were not pushed in the same direction. In Panel B and of Figure 2 we see their levels of previous experience declining around this time. If government service had become more pertinent to Senate races, then perhaps experienced candidates were drawn to those rather than House contests. The regression lines, corrected for autocorrelation, reveal significant and fairly dramatic differences between the two bodies. Direct election may have increased the degree to which senators had previous government experience, but in the House, the opposite occurred. The results are consistent with these interpretations, but there is no way conclusively to tie changes to the 17th Amendment.

CHANGES IN LEVEL OF RESPONSIVENESS

Membership composition was not all that reformers hoped to change through direct election. Many observers believed that senators would become more responsive to changes in public sentiment. John H. Mitchell (1896, 395) noted "a belief in the public mind that proper deference is not given by the Senate to the demands and interests of the people." He went on to remark that elections by the people rather than the legislatures would quite likely solve the problem. Making the Senate more sensitive to fluctuations in the public mood was a common goal of reformers, and in this section we determine whether that was accomplished.

Greater sensitivity could not be secured if the membership remained unchanged, so the first question is whether direct election stimulated legislative turnover. In the short run, it certainly did. If the focus of attention is strictly on the amount of turnover caused immediately by the transition, then there is little doubt that many Senate careers were disrupted. We determined the date of the initial direct election in all 48 U.S. states. This ranged from 1908 (for Oregon's Class 3 elections) to 1918 (for states having Class 2 elections and not adopting direct election prior to the 17th Amendment). We then determined the amount of turnover at this intersection of indirect and direct election, uneven as it was across the country.

Out of 96 cases occurring in these first direct elections from 1908 to 1918, the previous incumbent survived in 54 direct elections (56%). Of the remainder, 13 incumbents ran and lost in the general election, 8 ran and lost in the primary, and 14 chose not to run; 7 seats involved vacancies.[3] In other words, the first direct election produced 42 winners (44%) who had not been senators the previous term. To provide some sense of perspective, the mean percent of new members in the six actual Senate election years from 1908 to 1918 inclusive was 21.5, substantially and significantly below the 44 percent mentioned above. Clearly, a large portion of the indirectly elected Senate was not prepared to cope with direct election.[4]

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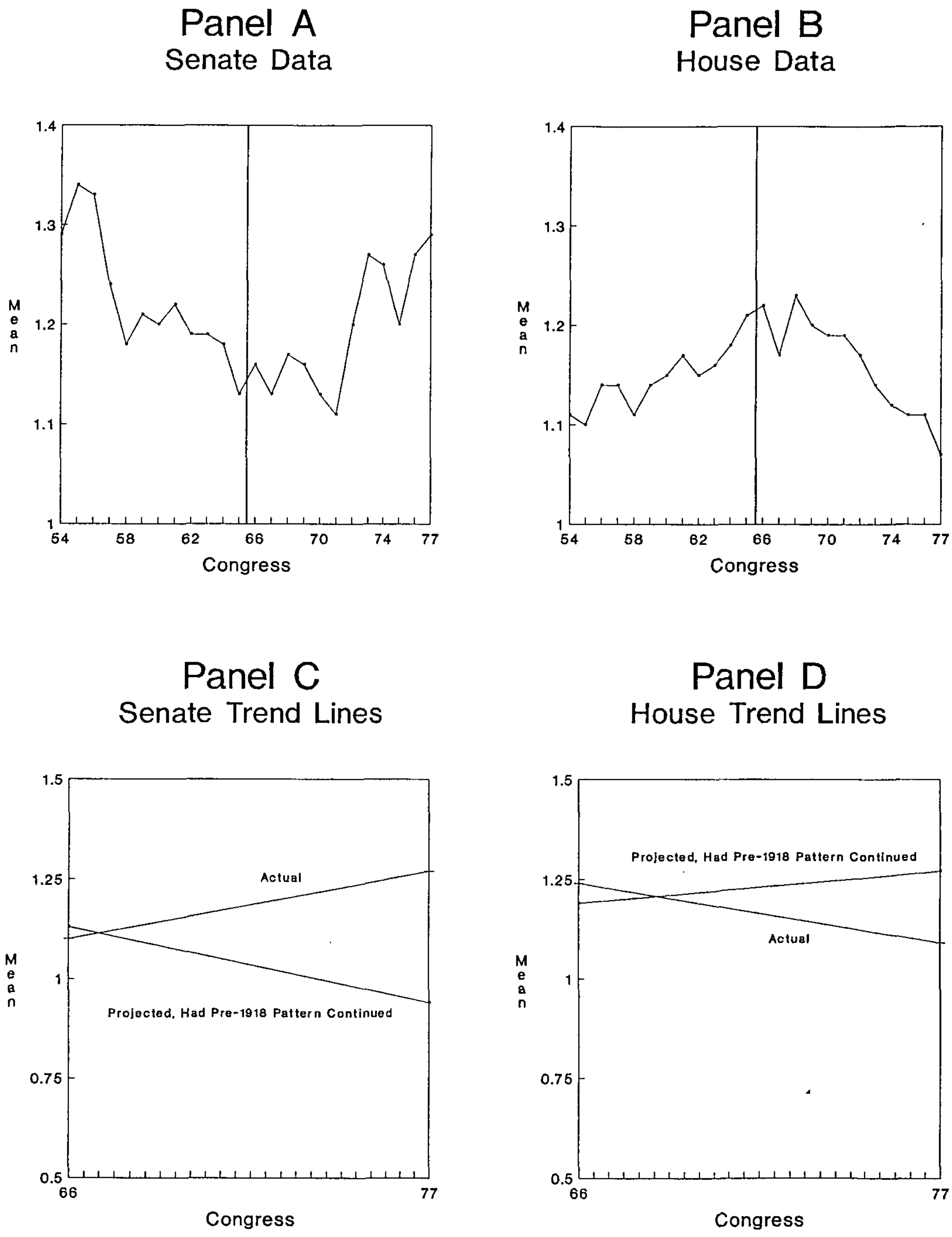
[2] Members with no previous government service were coded 0; those with experience at the local level only were coded 1; those with state-level experience were coded 2; and those with federal-level experience were coded 3. The *Biographical Directory of the American Congress, 1789–1989*, records no information on the length of time spent in each previous position.

[3] Several of these involved short-term appointees who could be viewed as incumbents and could have run for the ensuing full term. Therefore, the number choosing not to run may exceed 14.

[4] It should be noted, however, that after this initial turnover the long-term effect of direct election may have been to increase career length. It is difficult to make confident statements about cause and effect, since congressional stays were lengthening generally during this period, but the results suggest a more dramatic increase in the Senate than in the House. Just as voters apparently were more eager than state legislators to have senators with previous government experience (see Figure 2), they also seemed slightly more attracted to candidates with *congressional* service (a sentiment not in evidence among modern voters).

FIGURE 2. Mean Level of Pre-Congress Government Service



Source: Cleaned version of ICPSR 7803, *Biographical Directory of the U.S. Congress*, 1789–1989. See unnumbered note on page 845.
Note: In Panel C the "actual" line represents the pattern produced by direct election while the "projected" line is an estimate of the likely pattern had indirect election continued. The vertical line in Panels A and B represents the advent of universal direct election in the Senate.

RAPIDITY OF PARTISAN CHANGE

A better way to evaluate responsiveness is the speed with which partisan change occurred in the Senate. The logic of the founders was that the Senate as a body would be slower to move than the House. In this manner, the Senate would "protect the people against the transient impressions into which they themselves might be led" (Madison [1840] 1987, 193). With indirect election, plus lengthy and staggered terms, senators would be less susceptible to popular whims. Current passions would be more directly and immediately reflected in the composition of the House, but the Senate would afford officials the opportunity to reflect upon whether or not these passions were more than passing fancies.

The original constitutional design envisioned that a change of majority party in the House would be followed (if the "new" direction were deemed widely desirable) by a similar change in the Senate or (if undesirable) by a reversion of the House to the previous majority party while the Senate impassively watched the stops and starts of the House. Since the 17th Amendment removed a key factor insulating the Senate from "whims," we hypothesize that its enactment should alter the patterns anticipated by the founders. That is, with both bodies being popularly elected, partisan change is more likely to be simultaneous rather than lagged (or nonexistent in the Senate).

If we examine the 50 years both before and after enactment of the 17th Amendment, then we find an instructive contrast. From 1862 to 1912, there are five instances of the constitutionally anticipated pattern. In four cases a party switch in the House was parroted two years later in the Senate (Republican to Democrat in the 1876 House and 1878 Senate; Democrat to Republican in the 1880 House and 1882 Senate; Republican to Democrat in the 1890 House and 1892 Senate; and Republican to Democrat in the 1910 House and 1912 Senate). In one case the House switched twice—from Republican to Democrat (1882) and back again (1888) before the Senate moved at all. Remarkably, during this entire period, only once did a party switch not follow the foreseen sequence: In 1894 the House and Senate simultaneously changed from a Democratic to a Republican majority.

During the 50 years after 1913, the case record is just the opposite: one in the anticipated pattern and five exceptions to it. The House switched to a Democratic majority in 1930, two years before the Roosevelt realignment, but the Senate did not follow suit until FDR's election in 1932, thus reproducing the pattern of cautious Senate movement intended by the founders. But, outside of this series of events, no other instance of party switching subsequent to enactment of the 17th Amendment has given any indication of the Senate being more "cautious." In 1918, 1946, and 1952, both chambers simultaneously switched from a Democratic to a Republican majority, and in 1948 and 1954 both did the reverse.

Before 1913 the Senate usually fulfilled its intended role of lagging behind or standing pat, but after 1913 it almost always moved in lockstep with the House. In the 1980s there was even the unprecedented phenomenon of the Senate moving and the House standing pat. For better or worse, the Senate lost an important insulating agent—indirect elections. Causation cannot be proved, but it appears the 17th Amendment played a role in making the Senate a more rapidly responding institution (see also Alford and Hibbing 1989).

RESPONSIVENESS TO PUBLIC SENTIMENT

Another approach is to determine whether a change in the share of partisan seats (not necessarily entailing a switch in majority) is closely tied to changes in the voting behavior of the people. This information is difficult to obtain for periods predating modern survey techniques. Still, with a few assumptions it is possible to approximate a test of the hypothesis that the Senate became more responsive to public opinion after the advent of direct election.

The most important assumption we make is that a shift in the popular vote for president reflects a shift in general partisan preferences. While that assumption is not without problems, it is more accurate for the earlier part of this century than today. At issue is whether shifting partisan preferences in presidential races are reflected in the partisan distribution of Senate seats. We hypothesize that direct election will produce a tighter match between these shifts. In other words, shifts in presidential voting will not do a good job of predicting shifts in the Senate before the 17th Amendment but will do a better job afterward. This is based on the notion that state legislators were not particularly sensitive to public sentiments when they selected senators.

To test this hypothesis, we recorded shifts in the partisan distribution of both presidential voting and seats in the Senate and the House. For example, between 1860 and 1864, the two-party vote for the Republican candidate for president (Abraham Lincoln) declined by 2.4%. At the same time, the share of Republican seats increased by 5.2% in the Senate and by 7.1% in the House.

Since only one data point is available every four years, we extended the period analyzed beyond that used in some portions of this study. We cover 1860 to 1964 but exclude 1908–16, both because the transition to direct election occurred then and because 1912 was an extremely unusual presidential election. It is the only presidential election since the Republican Party was formed in which the Democratic and Republican nominees did not finish 1–2 in the presidential contest. Even though we use percentage of the two-party vote in the analysis, the disruption owing to Theodore Roosevelt's third-party candidacy is unprecedented and misleading (suggesting more Democratic strength than actually existed). These exclusions create two separate data sets; one running from 1860 to 1908 and the other from 1916 through 1964.

While conceptually cleaner, blocking out the transitional years makes it inappropriate to treat the data in a single equation fashion as was done in conjunction with figures 1 and 2.[5] Treating the data as two separate time series, in turn, results in a loss of degrees of freedom and makes it impossible to offer firm statements about the significant levels of difference in equation parameters. But this procedure should be sufficient to give at least an indication of whether the connection between partisan shifts in presidential preference and Senate seats became tighter as a result of the 17th Amendment.

Once again, the House is used as a control for historical changes that may come into play. Our hy-

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[5] Viewing the dummy as reflecting the new intercept, for example, is not entirely accurate, since a significant gap exists in the time series. Equal spacing of data points is necessary in continuous time series.

TABLE 1. Responsiveness of House and Senate to Partisan Shifts in Presidential Election: Before and After Direct Election in the Senate

	% Change in Republican Senate Seats, *t*-4 to *t*		% Change in Republican House Seats, *t*-4 to *t*	
	Before 1912	After 1912	Before 1912	After 1912
Constant	−.65	−.23	.51	−.46
(s.e.)	(2.46)	(2.24)	(3.17)	(2.48)
b	.75	1.09*	1.93*	1.33*
(s.e.)	(.65)	(.26)	(.84)	(.28)
Adj. R^2	.03	.61	.29	.66
F	1.31	17.99*	5.30*	22.10*
D.W.	1.52	2.01	2.71	2.82
N	12	12	12	12

Note: The independent variable in all four regressions is the change in the Republican share of the presidential vote from *t*-4 to *t*.
* = $p < .05$

pothesis is that presidential election shifts will be more relevant to the partisan composition of the Senate after direct election was instituted, so we expect the post-1912 equations in the two chambers to be more similar to each other than are the pre-1912 equations. The best way to present the results is a series of four simple regression equations (Senate before; Senate after; House before; House after), as shown in Table 1.

As expected, the post-1912 equation in the Senate is much stronger than the pre-1912 equation. A shift of 1% in presidential vote toward the Republicans yields an increase of more than 1% in the Republican share of Senate seats compared to the previous election, a substantially larger shift than is predicted during the pre-1912 period (.75). Even at that, the pre-1912 coefficient fails to achieve statistical significance. More important, the presidential shifts do a much more accurate job of predicting shifts in the partisan composition of the Senate after 1912. Presidential election shifts explain only 3% of the variance in Senate shifts, prior to the 17th Amendment, but afterward they account for 61% of the variance; the *F*-statistic is significant after and not before.

But perhaps presidential elections simply became more relevant to congressional politics as the century progressed. If so, then we should find a pattern of results in the House similar to that in the Senate. The last two equations of Table 1 indicate that this is not entirely the case. As expected, presidential elections are much more predictive of House than of Senate composition in the earlier period. Later, despite becoming somewhat more determinative of House composition (although the size of the regression coefficient is actually reduced by nearly one-third, from 1.93 to 1.33), presidential elections are no more able to predict House shifts than Senate shifts.

The most important conclusion from this portion of the analysis is that Senate elections once were nearly completely detached from presidential politics. With the advent of the 17th Amendment, the contours of the Senate came to be shaped by shifts in partisan performance in presidential races in the same manner as the House (compare the R^2 for the post-1912 House and Senate). This has had lasting consequences, and in fact discussion has intensified in recent decades over the hypersensitivity of the Senate.[6] No doubt much of this was bound to happen anyway, but the change to direct election certainly is implicated in the Senate's abrogation of its duty to be the "saucer that cools the coffee" (see Fenno 1982, 5).[7]

CONCLUSION

Woodrow Wilson (1885, 3) noted that "the Senate is just what the mode of its election and the conditions of public life in this country make it." If Wilson's contention is accurate, then a change in the mode of election should produce changes in the Senate (holding constant the "conditions of public life"). This study provides evidence that a shift in the mode of election did lead to changes in the Senate. Clearly observable consequences flowed from the 17th Amendment. These can be seen in the kinds of people who became senators as well as in the degree to which the Senate is responsive to the public mood. It is also likely that, as a result of the shift to direct election, both the Senate and the state legislatures experienced change in structure, procedures, and institutional position, although at this point we lack the data to offer detailed evidence of these purely procedural/institutional changes.

The findings we *do* present support the conclusion that the shift from indirect to direct election had implications for many facets of the Senate. Traits of typical senators changed in such a way as to suggest that, relative to the preferences of state legislators, ordinary people desired someone with government experience rather than wealth and family connections. Change also appeared in the manner the Senate responded to fluctuations in public sentiment. Specifically, it reacted to the popular mood with more sensitivity and more rapidity. For better or for worse, direct election rendered the Senate less sedate and more closely tied to the people, synchronizing it with the

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[6] Alan Ehrenhalt (1986, 583), for example, notes that "the modern Senate does not cool any passions. If anything, it heats them up.... The Senate has all the political hypersensitivity of the House if not more."

[7] For evidence on changes in the degree to which the Senate is responsive to state legislative partisan composition, see King and Ellis 1996. On changes in the swing ratio, see Stewart 1992.

House and the presidency; in fact, it often looks much like a smaller version of the House.

The populist reform agenda has popular support as enthusiastic today as it was 100 years ago during the buildup to the 17th Amendment. Ballot reforms and senatorial direct election have been replaced in the public eye with initiative votes and legislative term limits, but the impulse to empower the people at the expense of the elite (whether the elite are believed to be monied interests, as before, or "special" interests, as now) is consistently strong. Contrary to those who believe reforms are destined to failure and are unlikely to produce any significant change (see Bickel 1968, 3; Daynes 1971, vi; Haynes 1938, 1005), our results suggest that proposed changes should not be taken lightly. Arguments will continue as to whether the 17th Amendment had good or bad consequences, but it is abundantly clear that changes did occur as a result of direct election. Institutions like the Senate may not be total creatures of their "mode of election," but they are at least partially shaped by it. In one sense, it is encouraging that political institutions are not carved in stone, but the very malleability we have underscored makes it essential for the public to consider carefully the reforms for which it agitates.

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The "Veepstakes": Strategic Choice in Presidential Running Mate Selection

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We examine the presidential nominees' choice of running mate in each election since 1940, when Franklin Roosevelt established a precedent by naming his own. To analyze the 22 choices made from the pool of 127 serious possibilities, we employ a discrete choice model. We find that the presidential nominee's choice is explained primarily by the size of the prospective vice president's state, by whether the running mate finalist was a rival for the nomination, and by the balance in age for the ticket.

> In electing a subordinate officer the Electors will not require those qualifications requisite for supreme command. The office of Vice President will be a sinecure. It will be brought to market and exposed to sale to procure votes for the President.
>
> —William Cocke of Tennessee, debating enactment of the Twelfth Amendment, December 2, 1803 (quoted by Witcover 1992, xiii)

> If it is impossible to find one person who combines within his or her heritage, personality, and experience all the virtues allegedly cherished by American voters, the parties console themselves by attempting to confect out of two running mates a composite image of forward-looking-conservative, rural-urban, energetic-wise leadership that evokes hometown, ethnic, and party loyalties among a maximum number of voters. That, at least, is the theory behind the balanced ticket.
>
> —Nelson Polsby and Aaron Wildavsky (1991, 168)

In a quadrennial ritual, a beaming presidential nominee assures all who will listen that his newly chosen running mate is "the best person for the job." Realistically, though, just as those who opposed the Twelfth Amendment foresaw, the outcome of the "veepstakes" is far more likely to be based on short-term electoral calculations than on long-term governance considerations. As an aide to Vice President Hubert Humphrey put it: "Once the election is over, the Vice President's usefulness is over. He's like the second stage of a rocket. He's damn important going into orbit, but he's always thrown off to burn up in the atmosphere" (quoted by Light 1984, 11). Presidential nominees seek to select, from a pool of potential running mates they deem at least minimally qualified to serve, the one who will provide the greatest boost to their chances of being elected.

In this quest to find the most useful running mate available, presidential nominees are showered with advice, most of which is reducible to a simple formula: Balance the ticket. The strategic importance of balance dominates—indeed, virtually monopolizes—discussions of vice presidential selection (e.g., Goldstein 1982; Natoli 1985; Nelson 1988a and b; Polsby and Wildavsky 1991; Pomper 1963). For example, a liberal is supposed to opt for a moderate or perhaps even a conservative running mate, thereby extending an olive branch to other factions of the party and reaching out to the electorate at large. By the same token, a westerner is expected to choose an easterner, an "outsider" to settle on an "insider," an elderly nominee to select someone young and presumably vigorous, and a young nominee to pick someone older and presumably more experienced.

Like so many other pieces of conventional political wisdom, this one is a composite of what seems rational from the perspective of the decision maker, beneficial from the perspective of the political system, and typical from the perspective of the historical record. Our purpose here is to determine whether a set of hypotheses based on it can account for the selection of running mates during the modern era.

THE MODEL

The modern era of vice presidential selection began in 1940. Before then, the running mate was chosen by party leaders, and the presidential nominee was unlikely to participate actively in the process. At the 1896 Democratic convention, presidential nominee William Jennings Bryan would not even let his Nebraska delegation vote on the running mate, so eager was he to avoid internecine party battles (Polsby and Wildavsky 1991, 168).

As the 1940 election neared, it was clear that if Franklin Delano Roosevelt decided to seek a third term, then he would need to replace Vice President John Nance Garner, who had publicly opposed his policies and had begun campaigning against his reelection. After much dithering, Roosevelt settled on Secretary of Agriculture Henry Wallace. Finding him too bitter a pill to swallow, Democrats at the convention threatened to deny Wallace the nomination, but Roosevelt countered by proclaiming that he would refuse to run. Cowed, the convention capitulated, and the precedent of following the presidential candidate's lead in the selection of a running mate was set (see, e.g., Witcover 1992). On only one occasion thereafter (in 1956, when Adlai Stevenson threw the choice open to the delegates) has the convention vote amounted to much more than a formal ratification of the decision made by the presidential nominee and his advisors.

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As noted above, the presidential nominee's interests are generally defined in terms of getting elected rather than governing once elected, and the ideal running mate is therefore the best electoral complement. The practical implication is that the running mate should not be too similar. Thus, Bill Clinton's choice of another young, southern, centrist Democrat in 1992 was widely seen as a risky shift away from the balancing principle. By contrast, even though the choice of Lloyd Bentsen in 1988 prompted pundits to observe that the Democratic vice presidential nominee seemed to have more in common with George Bush than with Michael Dukakis, the selection of Bentsen was a textbook example of ticket balancing. He was virtually everything Dukakis was not: elderly, conservative, a consummate insider, a southerner, and a WASP.[1]

What, then, should a presidential nominee and his advisors look for in a running mate? From the balancing perspective, there is no single set of desiderata; the ideal choice is not necessarily middle-aged, for example, nor ideologically moderate, nor from a border state. Rather, the answer depends on the characteristics of the presidential nominee. It follows that in order to understand ticket balancing, we must focus not just on the characteristics of potential running mates but on how these relate to the presidential nominee. Of course, no model can capture the full richness and complexity of these characteristics or the tradeoffs made among them in a changing set of electoral circumstances and with a changing cast of characters. Still, with data from the historical record, we can consider the main elements of running mate choice.

One characteristic is region. Some presidential nominees—Jimmy Carter, among others (Rosenstone 1983, 64–5)—have been so concerned about geographical balance that they have refused even to consider anyone from their own region. Others have considered potential running mates from all over the country but have tried, if possible, to select someone whose regional base complemented their own.

Demographic balancing is also possible. Perhaps the most dramatic example is the strategy pursued by Walter Mondale in 1984, when he set out to diversify the ticket in terms of gender and race or ethnicity by considering such a lengthy parade of minorities and women that critics derided the process as, among other things, "the running-mate 'Gong Show' " and "a PR cattle call" (Brookhiser 1986, 154–5). Obviously, this type of ticket balancing is less firmly established than other strategies; indeed, in many quarters the selection of a woman or a racial or ethnic minority is still seen as unacceptably risky. Accordingly, compared to others under serious consideration for the vice presidential nomination, there is no guarantee that women or racial/ethnic minorities will find themselves at a competitive advantage. Nonetheless, these factors are among the potential balancing blocks that presidential nominees and their advisors can take into account when putting together a ticket, although whether they will constitute a plus or a minus should be regarded as an open question.

Looming even larger in most considerations of ticket balancing is religion. When, as is generally the case, the presidential nominee is a Protestant, one way to enhance the ticket's electoral appeal is to select a non-Protestant running mate. In 1996, for example, Bob Dole's attraction to John Engler and Connie Mack stemmed in part from their Roman Catholicism (Balz 1996). That obviously was not the deciding factor, since Dole chose Jack Kemp. Still, religious balancing is often viewed as a key not only to who is seriously considered but also to who is chosen.

We have already commented on age balancing, the strategy of counterposing an older presidential nominee and a youthful running mate, such as George Bush and Dan Quayle, or vice versa, such as Dukakis and Bentsen. Bush remarked on his choice that, "for me, this is a statement of confidence in a younger generation . . . in a whole new generation being represented on the national scene" (Boyd 1988), though he may simply have wanted a running mate who would not overshadow him.

A related strategy is to use the second spot to shore up perceived shortcomings in the presidential nominee's political experience. Over the past half-century, numerous presidential candidates have campaigned as "outsiders" determined to "clean up the mess in Washington" or "get government off our backs." According to the conventional wisdom, the ideal complement to an outsider is an insider; thus, as Michael Nelson (1988b, 862) has noted, since World War II the vice presidential candidate has often been the member of the ticket with more federal government experience. Jimmy Carter (1982, 35), for example, considered it vital "to choose a member of Congress as my running mate in order to provide some balance of experience to our ticket. Without ever having served in Washington myself, I needed someone who was familiar with the federal government and particularly with the legislative branch." By the same logic, a presidential nominee well versed in the ways of the nation's capital presumably would have greater latitude to select a complete outsider than would a candidate who lacked experience inside the Beltway.

Battles for the presidential nomination often pit one ideological wing of a party against another. Eisenhower versus Taft in 1952, Humphrey versus Kennedy and McCarthy in 1968, and Dole versus Buchanan in 1996 are examples of struggles for ideological supremacy within a party as well as personal competitions. For a party to triumph in the fall campaign, factions that were torn asunder during the spring and summer must be rejoined, and the selection of a running mate provides an opportunity to do just that. Balancing the ticket ideologically has the potential to reconcile disaffected elements of the party and to broaden the presidential nominee's appeal. Of course, selecting a running mate from a different wing of the party can also occasion considerable awkwardness, as champions of contrasting political ideas scramble to patch over the differences between them. Jack Kemp's haste in August 1996 to get in step with Bob Dole is the most recent case in point. The victors also may be more concerned about consolidating their party control than about reconciling differences, in which case their motivation to construct an ideologically "pure" ticket is likely to prevail over their motivation to reach out. For these reasons, it is not always clear whether being from a different wing of the party constitutes an advantage or a disadvantage for potential running mates, but it does seem certain that their stand on the issues will be taken into account.

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[1] Among Bentsen's other charms was his residence in a mega-state that had voted Republican in recent presidential elections but which, it was hoped, might be nudged into the Democratic column in 1988.

A similar strategy is to offer the second spot as a consolation prize to a defeated rival for the presidential nomination. The pluses and minuses of this strategy closely parallel those of ideological balancing, but another element is added to an already volatile mix—the residue of personal bitterness from months of high-stakes competition. Indeed, during the modern era, losing out for the top spot may dim one's vice presidential prospects. Reiter (1985, 123–4) argues that in recent decades the removal of the process from the hands of party bosses has undermined cooperation among party members, making it "harder for presidential nominees to swallow the hurt caused by the attacks of their vanquished foes and ask one of them to join the ticket, and . . . harder for the foe to agree to serve."

Finally, the temptation to "go hunting where the ducks are" is virtually irresistible for an electorally motivated politician. In the context of choosing a running mate, this means that it is more blessed to be from a large state than a small one. A vice presidential nominee who can help capture the electoral votes of a mega-state may determine the outcome of the election. Yet, the home state advantage is questionable. Rosenstone (1983, 87) estimates that in elections between 1948 and 1972 voters in the vice presidential nominee's home state were 2.5% more likely to vote for that ticket than they otherwise would have been. Dudley and Rapoport (1989), surveying the 1884–1984 period, find a mere three-tenths of one percentage point advantage and conclude that vice presidential nominees are seldom decisive in carrying their home state (but see Holbrook 1994, Wattenberg 1995). The crucial issue, however, is not whether the advantage really exists but whether presidential nominees and their advisors think it does. Entirely aside from considerations of balance, then, the selection of a running mate seems likely, ceteris paribus, to fall on someone with the potential to deliver a key state.

DATA AND METHODS

Before we can determine why presidential nominees and their advisors choose one person instead of several others, we must identify the pool of finalists and which one gets the nod. Neither of these is a simple matter. To test the criteria we have just outlined, we must specify measures of the characteristics of finalists and of the presidential nominees who have considered them.

The Finalists

As Goldstein (1982, 322–3) notes, determining who is seriously considered for the vice presidential slot in a given year can be difficult, for there is no definitive, "official" source of such information. Goldstein (1982) compiled such lists for 1952 through 1980. Our search of source materials upheld the validity of those lists, so we adopted them here.[2] Our remaining task was to extend the lists back to the beginning of the modern era, 1940, and forward to the most recent election year, 1996. We drew heavily on contemporary media reports (especially in *Newsweek*, *Time*, and the *New York Times*, for June, July, and August of each presidential election year); political biographies, memoirs, and oral histories (e.g., Martin 1977, Nixon 1979, Smith 1982); and numerous other sources (most notably Witcover's [1992] history of vice presidential selection and performance).

Each year, many names were floated, sometimes by the presidential nominee or advisors, sometimes by the media, sometimes by the would-be vice presidents themselves. Our intent was not to catalogue anyone ever discussed but only those under serious consideration when the invitation was issued—the "finalist pool" (see Table 1). Overall, by combining Goldstein's lists and our own, we spanned 22 selections involving 127 finalists (some of whom appeared on more than one list) over 14 election years. We excluded the six occasions on which an incumbent was kept on the ticket (Nixon in 1956, Agnew in 1972, Mondale in 1980, Bush in 1984, Quayle in 1992, and Gore in 1996),[3] along with the two occasions on which the running mate was selected, in fact as well as in form, by the party convention rather than the presidential nominee (McNary in 1940 and Kefauver in 1956).

The Chosen

Having identified the finalists, it remained to identify which one was chosen. At first glance, this may seem simple, for there is no mystery about who receives the nomination. If we had used the announced nominee, then only the 1972 Democratic ticket would have been a problem (Eagleton was replaced by Shriver). But we rejected that approach because often the running mate was not the first choice. In 1944, for example, only after Earl Warren twice spurned the offer did Dewey accede to putting John Bricker on the ticket (see, e.g., Smith 1982, 396). Similarly, in 1968 Humphrey turned to Muskie apparently only after Edward Kennedy had forsworn interest and Nelson Rockefeller had declined Humphrey's invitation (Goldstein 1982, 57, 63). We

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[2] The one exception is Edward Kennedy, who appears on Goldstein's 1968 list. Although it seems clear that the nomination was his if he wanted it (Goldstein 1982, 57), Kennedy repeatedly avowed that he did not want it. Colin Powell was in the same position in 1996: not a finalist because he took himself out of the running.

[3] In 1956 and 1992 there was considerable public discussion of the possibility that the incumbent vice president would be dumped, but these movements did not progress far enough for Nixon or Quayle to be pitted against others in a final round of decision making (see, e.g., Sirgiovanni 1994).

TABLE 1. The Finalist Pool, 1940–96

Year	Democratic Finalists	Republican Finalists
1940	Barkley (M) Byrnes (C) W. Douglas (L) Hull[a] (C) H. Wallace[b] (L)	*McNary*
1944	Barkley (M) Byrnes (C) W. Douglas (L) Truman[a,b] (M) H. Wallace (L)	Bricker[b] (M) Warren[a] (L)
1948	Barkley[b] (M) W. Douglas[a] (L)	Bricker (C) Halleck (C) Stassen (M) Warren[a,b] (L)
1952	Barkley (M) Chapman (M) Fulbright (M) Kefauver (L) Kerr (C) Magnuson (L) Monroney (M) Russell (C) Sparkman[a,b] (M)	Halleck (C) Judd (C) Knowland (C) Langlie (M) Nixon [a,b] (C) Thornton (M)
1956	*Kefauver*	*Nixon*
1960	Freeman (L) Humphrey (L) H. Jackson (M) Johnson[a,b] (C) Symington (M)	R. Anderson (C) Ford (C) Judd (C) Lodge[b] (M) Mitchell (M) T. Morton (M) N. Rockefeller[a] (L) Seaton (C)
1964	Humphrey[a,b] (L) R. Kennedy (L) McCarthy (L) McNamara (M) Mansfield (M) Shriver (L)	Ford (C) Miller[a,b] (C) Scranton (M)
1968	Harris (M) R. Hughes (L) Muskie[b] (M) N. Rockefeller[a] (M) Sanford (M) Shriver (M)	Agnew[b] (M) H. Baker (C) Finch[a] (L) R. Morton (M) Volpe (M)
1972	Askew (L) Church (L) Eagleton[b] (L) E. Kennedy[a] (L) Mondale (L) Nelson (L) O'Brien (M) Ribicoff (L) Shriver (L) White (L)	*Agnew*
1976	Church (L) Glenn (M) H. Jackson (M) Mondale[a,b] (L) Muskie (L) Rodino (L) Stevenson II (L)	Armstrong (C) H. Baker (C) R. Dole[a,b] (C) Ruckleshaus (M)
1980	*Mondale*	H. Baker (C) Bush[b] (M) Ford[a] (M) Kemp (C) Laxalt (C) Lugar (M) Rumsfeld (M) W. Simon (C) Vander Jagt (C)
1984	Bentsen (C) T. Bradley (L) Cisneros (L) M. Collins (M) Dukakis (L) Feinstein (M) Ferraro[a,b] (L) Goode (L)	*Bush*
1988	Bentsen[a,b] (C) Gephardt (L) Glenn (M) Gore (M) Graham (M) Hamilton (M)	Alexander (M) Deukmejian (M) R. Dole (M) Domenici (M) Quayle[a,b] (C) Simpson (C) Thompson (M)
1992	Gore[a,b] (M) Graham (M) Hamilton (M) Kerrey (M) J. Rockefeller (L) Wofford (L)	*Quayte*

Note: For the six occasions on which the incumbent vice president was left on the ticket and the two occasions on which the choice was thrown open to the convention, the name is given in italics but is excluded from the analysis.

The presidential nominees and their ideological categorizations as liberal (L), moderate (M), or conservative (C) are as follows: 1940—Roosevelt (L); 1944—Roosevelt (L), Dewey (L); 1948—Truman (M), Dewey (L); 1952—Stevenson (M), Eisenhower (M); 1960—Kennedy (M), Nixon (M); 1964—Johnson (L), Goldwater (C); 1968—Humphrey (M), Nixon (M); 1972—McGovern (L); 1976—Carter (M), Ford (C); 1980—Reagan (C); 1984—Mondale (L); 1988—Dukakis (L), Bush (M); 1992—Clinton (M); 1996—Dole (M).

[a] The first pool member invited to become the running mate. Initials are included in some instances to distinguish a finalist from another prominent political figure with the same surname.

[b] Actual nominee.

considered it inappropriate to designate someone like Bricker or a Muskie the winner of the pool simply because he ended up on the ticket. These were not isolated instances. Eight of the 22 individuals we designate as the first choice did not become the running mate.[4]

An alternative approach would have been to try to ascertain each presidential nominee's sincere preference. For example, in 1996 Dole openly yearned to have Colin Powell on the ticket (Woodward 1996), but Powell made it clear that he would not accept. We rejected this approach for two related reasons. First, it is often impossible to determine the sincere preference; indeed, sometimes the presidential nominee seems unsure (Roosevelt in 1944 being a prime case in point), and the choice boils down to strategic calculations. Second, the real issue is not who the true favorite is, but who is asked. Accordingly, trying to determine each presidential nominee's sincere preference would have been not only fruitless but also pointless.

We focused on identifying each presidential nominee's first choice, as distinct from the final choice or a sincere preference. By "first choice" we mean the first to whom an invitation was issued, whether it was accepted or declined. To make this determination, we returned to the source materials used to compile the lists of finalists; the Appendix describes these sources. The results (see Table 1), some of which may be surprising, represent the most reliable conclusions we could draw from these sources.

The Predictors

We extracted information about the home state, gender, race/ethnicity, date of birth, religious affiliation, career, and political orientation of each presidential candidate and vice presidential finalist from standard reference works.[5]

For the *Regional Balance* variable, we classified states as western, midwestern, southern, border, or northern. Each finalist whose home state was in the same part of the country as that of the presidential nominee was coded 0 to designate regional imbalance; those who came from a different region than the presidential nominee were coded 1, denoting regional balance.[6] Geography also came into play in the *Size of State* variable, which we expressed as the number of electoral votes for a finalist's home state divided by the total number of electoral votes that year.

Several demographic variables were featured in the model. We pooled information on gender and race/ethnicity to categorize each presidential nominee and vice presidential finalist as either a member of a politically underrepresented group (a woman, an African American, or a Hispanic) or not. Then, as with regional balance, we created a dummy variable indicating *Demographic Balance* (1) or imbalance (0) within a potential ticket. Because every Democratic and Republican presidential nominee has been a non-Hispanic white male, this variable effectively indicates whether a finalist was a non-Hispanic white male (0) or not (1). A fourfold classification (Protestant, Roman Catholic, Jewish, or Greek Orthodox) provided the basis for a dummy variable coded 0 if the presidential nominee and would-be vice president were coreligionists, or 1 if the ticket was *Religiously Mixed*. Finally, *Age Balance* was coded 0 if a finalist was born within ten years of the presidential nominee, 1 if the two were born at least a decade apart.

For *Ideological Balance*, we adopted (with a single exception) Goldstein's (1982) classification of conservative, moderate, or liberal.[7] For the periods 1940–48 and 1984–96, we referred where possible to interest group ratings of legislators and, more generally, to contemporary media descriptions of both views and intraparty sources of support. These classifications, few of which should be controversial, are shown in Table 1.

We classified finalists and presidential nominees as outsiders if, prior to the year in question, they had neither served in Congress nor occupied a high-level post in Washington, DC.[8] Based on these classifications, we created a dummy variable for *Insider-Outsider* indicating ticket balance (1), that is, either an insider presidential nominee with an outsider running mate or vice versa, and imbalance (0), that is, two insiders or two outsiders.

We also categorized the pairings according to whether they had been *Opponents for the Presidential Nomination*. Most (106 of the 127 pairs—83.5%) had never competed against each other for the top spot.[9] Of the 21 pairs who had, we distinguished between those who had been rivals for the nomination sometime in the past (4 pairs: Bricker and Dewey before 1948, Humphrey and Johnson before 1964, Ford and

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[4] The eight were Cordell Hull in 1940, William O. Douglas in 1948, Nelson Rockefeller in 1968, and Edward Kennedy in 1972 on the Democratic side, and Earl Warren in 1944, Nelson Rockefeller in 1960, Robert Finch in 1968, and Gerald Ford in 1980 on the Republican side. In only two cases does the documentary record provide a clear indication of why an offer was spurned. Finch considered himself too close to Nixon personally and a Californian besides (Strober and Strober 1994, 23), and Ford and Reagan ceased negotiations when they could not agree on the nature of their working relationship (Witcover 1992, 309–13).

[5] Especially useful were Southwick (1984) and various editions of *America Votes* (1956 et seq.), *Political Profiles* (1976–79), *Who's Who in America* (1940 et seq.), and Barone and Ujifusa's *The Almanac of American Politics* (1972 et seq.), along with Goldstein (1982).

[6] We treated California as Richard Nixon's home state and Texas as George Bush's.

[7] Goldstein (1982, 325) classified Nelson Rockefeller as a liberal. Within the 1960 GOP context, this is unquestionably accurate. As a potential running mate for Humphrey in 1968, however, Rockefeller clearly did not represent liberal Democrats, so in that pool we reclassified him as a moderate.

[8] By contrast, Busch (1997, 2) defines an outsider in ideological rather than geographic terms: one who "(1) is outside the corridors of power, in the sense either of holding no office or of residing outside the 'mainstream' or majority of his party, explicitly rejecting the party's leadership and dominant element; (2) serves as the spokesman or representative of a broader group (a political movement) outside the corridors of power; and/or (3) serves as the spokesman or representative of ideas, ideologies, or themes that challenge the dominant element in the party."

[9] In a few cases, a vice presidential finalist had been a contender but dropped out early. In such instances, we treated the presidential nominee and the finalist as nonrivals.

Reagan before 1980, and Kemp and Dole before 1996) and those who had contested the nomination during the year in question (the remaining 17 pairs, the most recent being Kerrey in 1992).

The Statistical Model

Our focus on the selection of 22 different running mates poses an unusual data configuration. There is nothing exceptional about a set of decision makers (say, voters) simultaneously choosing from a single set of alternatives (say, Bush or Dukakis in 1988), but in our case the alternatives considered by each decision maker (the 22 presidential nominees) differ from those considered by every other. For example, we seek to account for why Dewey's first choice as running mate in 1944 was Warren rather than Bricker and why Kennedy selected Johnson in 1960 over Freeman, Humphrey, Jackson, and Symington. Note that both the individuals and their number vary from one choice situation to another; the 127 finalists are unequally spread over the 22 instances, ranging from only two for Dewey in 1944 and for Truman in 1948 to ten for McGovern in 1972.

Given this data configuration, the discrete choice model is the appropriate analytical tool. In that model, decision makers (in this case, presidential nominees) select one from two or more alternatives (vice presidential finalists), each of whom has a set of attributes (e.g., Warren's residence in the West and lack of experience in the nation's capital, Bricker's residence in the Midwest and experience in the U.S. Senate, and so on). The point of the analysis is to model these choices as a function of the attributes of the alternatives available to each decision maker. The question is which alternative was the first choice; all the others are treated equally as not chosen.

More formally, the probability that an alternative will be chosen is conditioned upon the likelihood that another alternative will be chosen instead. This is reflected in the functional form of the model:

$$Prob[y_i = j] = \frac{\exp(\beta' x_{ij})}{\sum_{k=1}^{m} \exp(\beta' x_{ik})},$$

where $Prob[y_i = j]$ is the probability that a presidential nominee will select finalist j in election year i, β is the vector of coefficients to be estimated, x is the vector of characteristics unique to each finalist, k is each finalist considered by a given presidential nominee, and m is the number of finalists in a given presidential nominee's pool (Maddala 1983, 42). Thus, who is selected from the pool depends on the characteristics of each potential running mate, given the characteristics of all the other available choices.[10]

TABLE 2. Running Mate Selection Model Results

Characteristic of Potential Ticket	Logit Coefficient	Standard Error
Regional balance	−1.39	(1.18)
Gender/racial/ethnic balance	−.02	(1.71)
Religious balance	−.69	(.90)
Age balance	1.29*	(.79)
Ideological balance	.74	(.69)
Insider-outsider balance	−4.57	(1.52)
Rivals for this year's presidential nomination	−6.32**	(2.25)
Never rivals for presidential nomination	−4.81**	(1.79)
Size of home state	78.95**	(23.85)
Number of cases	127.00	
χ^2	31.17**	
Degrees of freedom	9.00	
Pseudo-R^2	.42	
Percentage correctly predicted	68.2%	
Proportional reduction in error	59.8%	

*$p < .05$; **$p < .01$.

RESULTS

The model performed relatively well, correctly predicting 15 of the 22 first-choice selections (68.2%), a 59.8% proportional reduction in error compared to the 4.6 correct predictions expected on the basis of chance (see Table 2). This is consistent with the idea that since 1940 the choice of a running mate has been shaped by factors included in the model. We should add that the model correctly predicts some choices that were considered surprising when they were made, including, most notably, Bush's selection of Quayle in 1988. Of course, the model also leaves several choices unexplained. So there *is* accounting for taste, but it is not a full accounting.

More specifically, the statistical results indicate, first, that the selection of a running mate from the finalist pool has not been significantly affected by several factors long considered influential. One is regional balance. As noted earlier, this may have kept some well-qualified candidates out of the pool, but finalists from the same region as the presidential nominee have not been at a substantial disadvantage. If anything, they have had an advantage, though the nonsignificance of the coefficient sounds a caution against making too much of this reversal of expectation.

Balancing on demographic or religious grounds also is not much in evidence. Since Kennedy's election in 1960 and the civil rights and women's movements of the last several decades, presidential nominees in both parties, but especially Democrats, have become less likely to consider only WASP males as running mates. But many are called, and few are chosen. Of the ten GOP finalists who were not WASP males (Anne Armstrong and nine Roman Catholic men), only one

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[10] Some technical aspects of the estimation can be dispensed with here. We treated those who had been rivals in a past campaign as the excluded or reference category for interpreting the estimated coefficients for those who had never been rivals and those who were rivals only during the year at issue. Because of the uncertainty we expressed earlier about whether certain types of balance positively or negatively affect a finalist's chances, we employed two-tailed significance tests for some of the coefficients: the gender and race/ethnicity variable, the ideological balance variable, and the two presidential campaign rivalry variables.

(William Miller in 1964) was asked to join the ticket. Of the 22 Democratic finalists who were not WASP males (19 non-Protestants, two African Americans, one Hispanic, and three women, with some overlap), just two (Edward Kennedy in 1972 and Geraldine Ferraro in 1984) were the first choice. Because every presidential nominee except Kennedy in 1960 and Dukakis in 1988 has been a WASP male, it should be obvious that differing from the head of the ticket in these respects has not been a significant advantage for would-be running mates. Even though presidential nominees have not been very likely to use the second spot to broaden the ticket demographically or religiously, in the modern era they at least have become more likely than their predecessors to put a diverse set of finalists in the pool. To some extent this may be mere window dressing, though our effort to restrict the lists to finalists under serious consideration should be borne in mind.

Contrary to much conventional wisdom, the ideological balancing coefficient is also nonsignificant, indicating that in the modern era the idea of using the second slot to try to reconcile ideological wings of a party and thereby build a united front has often been honored in the breach. This is especially true of the Democrats, whose standard bearer on six of twelve occasions has chosen a running mate of similar ideological stripe.

This brings us to the determining factors. One is age: Being of an age cohort different from that of the presidential nominee has constituted a significant advantage for prospective running mates. How significant? The answer depends on the particular pool, but the selection of Bentsen in 1988 provides a good case in point. Given his characteristics and those of the five other finalists, the probability that Dukakis would choose him, according to our model, is .649 (see Table 3). If we counterfactually assume that Bentsen and Dukakis differed less than ten years in age, then the model-derived probability of selecting Bentsen falls to .337.

TABLE 3. Simulated Probabilities of Selecting Lloyd Bentsen as the Democratic Vice Presidential Nominee in 1988

Situation	Probability that Bentsen Would Be Chosen
Baseline (actual characteristics of Bentsen and Dukakis)	64.9%
Counterfactuals	
If Bentsen had been in the same age cohort as Dukakis	33.7%
If both Bentsen and Dukakis had been outsiders or insiders	99.4%
If Bentsen had been a rival for the presidential nomination in 1988	29.1%
If Bentsen and Dukakis had previously been rivals for the presidential nomination	99.6%
If Bentsen had been from Indiana (12 electoral votes) rather than Texas (29 electoral votes)	13.3%

Insider-outsider balancing also has affected running mate selection but not in the manner we anticipated. Other factors being equal, a finalist's ability to balance the ticket in insider-outsider terms has not had the expected positive effect.[11] Again, the Bentsen case can be used to gauge the magnitude of this tendency. If both Dukakis and Bentsen had been either outsiders or insiders, with no other outsiders in the running, the model would predict, with 99.3% certainty, that Bentsen would be the choice—well above the baseline 64.9% probability. This prediction obviously comes as a surprise, for there is hardly a hoarier piece of conventional wisdom than the need to balance the ticket in this regard.

How can we account for the fact that insider-outsider balance has been a net disadvantage for potential running mates? In the first place, our results certainly do not mean that outsiders have not wanted insiders on the ticket. In six of the eight cases of an outsider presidential nominee (75%), an insider was invited to fill the second slot (the two exceptions being Dewey in 1944 and 1948). But 13 of the 14 insider presidential nominees (93%, all except Nixon in 1968) invited an insider onto the ticket. For finalists with Washington experience, the chances of being chosen have actually been better with another insider heading the ticket. Moreover, among the 49 finalists considered by outsider presidential nominees, only six (12%) were themselves outsiders. Yet, since they were selected in two of the eight cases (25%), their chances were better than the outsider share of the finalist pool would suggest. Putting all this together, we conclude that the conventional wisdom about insiders and outsiders may hold, but only when forming the finalist pool, not at the selection stage.

Whether a particular finalist is asked to join the ticket also depends on rivalry for the presidential nomination.[12] The most favored finalists seem to be those who were competitors in a past contest for the nomination; three of the four actual running mates in this category (Humphrey in 1964, Ford in 1980, and Kemp in 1996) were first choices. By comparison, both those who had never competed against the presidential nominee and those who had vied in the just-completed battle were at a significant disadvantage. Only 17 of 106 finalists in the former category (16%) and 2 of 17 in the latter (12%) were first choices. Thus, if Dukakis and Bentsen had been opponents for the presidential nomination sometime before 1988, Bentsen's model-based chances of being selected by Dukakis in 1988 would have soared to 99.6%; but if they had competed that year only, Bentsen would have had just a 29.1% chance of being asked. Of course, our analysis reveals that this pattern exists but does not explain why. Nonetheless, someone who in the past mounted a challenge for the presidential nomination, accumulated national campaign experience, gained wide media exposure, and took steps toward building a following in the party—as opposed to campaigning within the last few months against the presidential nominee—may have distinct appeal for the standard bearer.

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[11] Because the insider-outsider balance coefficient was expected to be positive, we employed a one-tailed significance test. The unexpectedly negative coefficient obviously did not pass this test.

[12] Of course, such competition varies widely in the degree of bitterness engendered. The findings reported here are based solely on *whether* prospective running mates had been competitors for the presidential nomination, not on the character of that rivalry.

Finally, coming from a large state is a plus for finalists. For example, if Bentsen had not come from a mega-state, the model suggests that Dukakis may well have asked someone else. On the counterfactual assumption that Bentsen hailed from Indiana (known as the home of vice presidents) rather than Texas, his chances would have been only 13.3% rather than 64.9%. This is ironic in light of evidence, noted earlier, that the much-ballyhooed home state advantage of running mates is largely a myth; indeed, Dudley and Rapoport (1989) contend that the effect is minor and confined to small states. Presidential nominees either do not know this or ignore it.[13]

DISCUSSION

When tested against the historical record, the conventional wisdom fares poorly in some respects and well in others. Several factors that presidential nominees and their advisors are supposed to take into account—such as regional, religious, and ideological balance—do not appear to have significant influence on the selection of running mates in the modern era. Another—insider-outsider balance—does operate but not as expected. Yet others conform to expectations, especially the tendencies to balance age and to select someone from a state that can make a difference when the electoral votes are counted.

As noted above, in eight of the 22 choice situations considered here, the first person asked to be the running mate declined the invitation. How different would the results be if we focused on the people actually nominated instead of those who were first choice? To find out, we reestimated the model, excluding from the pool the first choices who declined the offer, to determine whether the model could identify the actual vice presidential nominee. The answer, in a word, is "No."[14] The performance of this reestimated model is unimpressive by any standard: $\chi^2 = 9.48$ on 9 degrees of freedom (n.s.); pseudo-$R^2 = .138$; correctly predicted = 40.0%; and proportional reduction in error = 26.9%.

The clear implication is that whereas a presidential nominee's first choice for running mate is fairly predictable, when that invitation is declined, the whole process is thrown into disarray. In the resulting scramble to patch together a second-best ticket, factors normally taken into account may well be ignored, while idiosyncratic factors (or at least factors not emphasized in the conventional wisdom) come to the fore. Accordingly, a choice process that looks fairly systematic when approached in terms of the initial invitations issued looks virtually random when approached in terms of the tickets actually formed. In any event, it appears that the factors normally guiding selection tend to go out the window once the first offer is spurned, perhaps because it is deemed important that the next choice be someone who will not decline.

It should be noted that the conventional wisdom, as represented in the model summarized in Table 2, does relatively well at predicting the first choices made by presidential nominees: It gets eight of the ten Republicans right, seven of the twelve Democrats. Even so, there are gaps, which leads us to wonder whether running mate selection is as electorally motivated as is ordinarily assumed. It seems plausible that the degree of electoral motivation depends on the presidential nominee's apparent chances of being elected. If the race looks close, then the temptation is to play it safe in choosing a running mate, hoping thereby to do as little damage as possible or, at best, eke out a small advantage.[15] If the prospects are extremely good or bad, however, then normal strategic considerations may be given less weight. With a large lead, there is more freedom to indulge personal preferences or find a compatible partner for what is hoped will be a long-term working relationship. When far behind, a dramatic choice may be made in an attempt to catch up; as one analyst remarked about Walter Mondale's "audacious" choice of Geraldine Ferraro in 1984, "prudent losers end up losers" (Brookhiser 1986, 155). Moreover, electoral considerations may mean little to nominees who would "rather be right than president"; in 1964, Barry Goldwater and his conservative followers probably never considered an ideologically incompatible running mate (Young 1974, 310), and one can hardly imagine George McGovern choosing a hawk in 1972.

Unfortunately, the modern era does not afford a sufficient number of cases to incorporate the closeness of an election or its ideological character into the model as contextual factors. Yet, one simple comparison does bring home the potential importance of electoral context. Earlier we noted that during the modern era there has been no significant tendency for presidential nominees to select an ideologically complementary running mate from the finalist pool. In contrast, in six of the eight (75%) choices in campaigns that appeared, at the time running mate selections were made, as though they would be highly competitive, the initial invitee was one who would bring ideological balance to the ticket. In the remaining races, which looked less competitive at the outset (though some were unexpectedly close, e.g., 1948 and 1976), only six of fourteen first choices (43%) offered ideological balance. So presidential nominees anticipating a tight race almost always have sought a complement, whereas those expecting a more one-sided contest have tended to ignore the ideological constraint. It seems that the positive or negative effect of ideology on the prospects of a potential running mate is closely tied to the electoral context—too closely tied for ideological balance to register an overall influence one way or the other.

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[13] Of course, running mates from larger states also may be chosen because they are more nationally prominent than finalists from small states.

[14] In this model, the number of cases is 117; the eight first choices were dropped, and two other names had to be excluded (Bricker in 1944 and Barkley in 1948) because only one candidate was left in the pool. In reestimating the model, we treated Eagleton as McGovern's running mate in 1972.

[15] A loss-minimization strategy would be consistent with Richard Nixon's often quoted dictum that a running mate can only hurt, not help, a presidential candidate. Yet, Nixon also can be quoted on the other side of the issue: In 1992 he speculated that Gore would give Clinton a five- or six-point lift in the crucial border states (Crowley 1996, 98).

The point is not that the conventional wisdom-based model tested here, which does not incorporate electoral context, is invalid, for it does quite a good job of predicting the first choice of presidential nominees for a running mate. Rather, the point is that such contextual factors as who is ahead, who is behind, and by how much surely matter, as do the geographical and ideological contours of the presidential nominee's support. Our understanding of the choice process can only be enriched by taking due account of the electoral context in which presidential nominees and their advisors plot strategy for the upcoming campaign.

APPENDIX: IDENTIFYING THE "FIRST-CHOICE" RUNNING MATE

The documentary record–contemporary news coverage, memoirs and biographies, histories of the vice presidency, and so on—provides a rich lode of anecdotal information about how particular tickets were put together. To identify the person initially offered the vice presidential nomination in the 22 choice situations considered here, we combed these sources carefully to try to identify the first person who actually received, from the presidential nominee or a designated representative, an invitation to be on the ticket.

Two interrelated caveats are immediately in order. First, the standard protocol seems to be that an explicit offer is not made until willingness to accept already has been signalled. That being the case, there can be a thin line between being sounded out and actually being offered the position. Second, such discussions are conducted in private and are subject to differing accounts and interpretations. In the absence of any unimpeachable source or ultimate validity check about who was the first choice, our task was to weigh the evidence and determine what the preponderance suggests. That is, we had to sift through numerous sources and, by triangulating the various accounts and conclusions, try to find the most credible answer.

This often involved judgment rather than hard-and-fast decision rules. Even so, in only two or three instances did it prove especially challenging to identify the first choice. One case was 1944, when Roosevelt openly encouraged several candidacies, including those of James Byrnes, William O. Douglas, and the incumbent Henry Wallace. But while the ailing, dispirited, and indecisive Roosevelt was sending out mixed signals, his closest advisors were determined that Truman should be chosen, and ultimately Roosevelt passively acceded (Young 1974, 213–31). The other two especially problematic cases occurred in 1968. On the Democratic side, we believe that most evidence points to Nelson Rockefeller as Humphrey's first invitee, but we grant that it is not altogether clear. On the Republican side, the standard account has Nixon considering several possibilities, including his long-time friend and trusted advisor, Robert Finch, but narrowing the choice to governors Volpe of Massachusetts and Agnew of Maryland (e.g., Witcover 1992, 219; Young 1974, 342–3). To the contrary, Finch claims he refused Nixon's repeated entreaties to join him on the ticket, and in his memoirs Nixon affirms that he offered the job to Finch (Aitken 1993, 355; Nixon 1979, 311–3; Strober and Strober

Year	*Democratic nomination*	*Republican nomination*
1940	Witcover (1992, 76); Young (1974, 177).	N.A.
1944	Witcover (1992, 85–7); Young (1974, 213–31).	"Bricker's Half a Loaf" (1944, 43–4); Smith (1982, 396); Williams (1956, 210).
1948	Williams (1956, 227–8); Witcover (1992, 103).	"Selected with Care" (1948, 23–4); Smith (1982, 500); Williams (1956, 229–30); Young (1974, 248).
1952	Goldstein (1982, 54–5); Martin (1977, 606–7).	Goldstein (1982, 54); Williams (1956, 241–3); Witcover (1992, 110–3).
1956	N.A.	N.A.
1960	Goldstein (1982, 62); Natoli (1985, 30–3); Witcover (1992, 150); Young (1974, 294–7).	Goldstein (1982, 63, 81); Witcover (1992, 137–8).
1964	Goldstein (1982, 56); Witcover (1992, 182–91).	Southwick (1984, 617–8); Young (1974, 307–12).
1968	Goldstein (1982, 57, 63); Young (1974, 335).	Aitken (1993, 355); Nixon (1979, 312–3); Strober and Strober (1994, 23).
1972	Goldstein (1982, 63); Natoli (1985, 24).	N.A.
1976	Natoli (1985, 36–7); Witcover (1992, 295).	Natoli (1985, 38–41); Witcover (1992, 287–9).
1980	N.A.	Goldstein (1982, 63); Witcover (1992, 309–13).
1984	Church (1984); Natoli (1985, 46–8); Witcover (1992, 321–3).	N.A.
1988	Kaus (1988); Stengel (1988).	Martz (1988); Shapiro (1988); Witcover (1992, 339–40).
1992	Goldman et al. (1994, 276–83); Shapiro (1992).	N.A.
1996	N.A.	Fineman (1996); Lacayo and Duffy (1996).

1994, 23). Bowing to the direct testimony of the two principals, we identify Finch, not Agnew, as Nixon's first choice.

Several histories of the vice presidency and of the selection process that synthesize materials from a wide array of sources proved invaluable in making these identifications (Goldstein 1982, Natoli 1985, Williams 1956, Witcover 1992, Young 1974). Our identifications of the first choices were especially influenced by the sources identified in the accompanying table. ("N.A." signifies "not applicable," either because the running mate selection was thrown open to the party convention or because the incumbent was left on the ticket.)

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Distance versus Direction: The Illusory Defeat of the Proximity Theory of Electoral Choice

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In an extensive series of articles, Rabinowitz, Macdonald, and Listhaug have launched a new, directional theory of electoral choice. According to their claims, the new theory is superior to the classical Downsian proximity model. Such a conclusion, however, is not tenable. First, it fails to take proper account of the lower falsifiability of the directional model. Second, it rests on empirical analyses that do not test the two theories on the predictions they actually make. Both are theories of individual choice, which implies intrapersonal comparisons of utility. Prior tests, however, have predominantly taken the form of interpersonal comparisons. A reanalysis based on the proper type of comparison yields an outcome diametrically opposed to that previously obtained. It also reveals an eccentricity effect that largely accounts for the apparent success of the directional model.

In an extensive series of articles, two of which were published in the *Review*, George Rabinowitz, Stuart Elaine Macdonald, and Ola Listhaug have launched a new spatial theory of electoral choice.[1] The new theory is presented as a direct alternative to the classical spatial model introduced by Downs (1957), on the basis of earlier work by Hotelling (1929).[2] Both models try to account for the same phenomenon (electoral choice) on the basis of the same information (the issue stands of voters and parties) but conceptualize the independent variable and, by implication, its effect in different ways.

Judging from the results hitherto presented, it would seem that the new theory has already superseded the old. In a large number of tests based on data from multiple countries and periods, Rabinowitz, Macdonald, and Listhaug consistently find their directional model superior to the classical proximity approach. This massive evidence notwithstanding, I shall argue that the superiority of the directional model is illusory rather than real. When properly evaluated, the proximity model remains the better theory.

My conclusion is based on a comparative evaluation of the two theories. Each theory is examined in the light of two basic criteria. The first is its *degree of falsifiability*, that is, the proportion of logically conceivable empirical outcomes that it rules out as incompatible with its propositions. A highly falsifiable theory has greater empirical content (i.e., it tells us more about reality) and is therefore more valuable than a less falsifiable one (Popper 1980, chapter 6). Yet, a high degree of falsifiability is obviously not the only hallmark of a good theory. Of great importance is the second criterion, namely, *the theory's degree of compatibility with empirical evidence*. In short, a good theory should maximize the risk of being proved wrong but nevertheless should prove right. The results of my examination indicate that the directional model is inferior to the proximity model in both respects.

My analysis proceeds in five steps. I begin by describing the main differences and similarities between the two theories. In so doing, I also spell out the implications for the first of the two criteria. The three subsequent sections focus on the second criterion. I first consider how the two theories have been, and should be, tested. Based on the conclusions from this section, I then reanalyze a significant portion of the data on which Rabinowitz, Macdonald, and Listhaug base their verdict. Finally, I specify the reasons why my results differ from those previously obtained. In the concluding section, I summarize my results and discuss their implications.

THE TWO THEORIES

The key distinction between the proximity and the directional model rests with the conceptualization of issue dimensions. In the proximity theory, issues are conceived as ordered and continuous sets of policy alternatives (e.g., a continuous set of alternative tax rates). An issue stand thus represents the preferred *position on a policy continuum*. In the directional theory, by contrast, policy alternatives are assumed to be dichotomous (e.g., low taxes versus high taxes), and issue stands are assumed to represent the *intensity* with which either of the two *sides* or *directions* is supported. The main argument advanced by Rabinowitz and Macdonald (1989, 93–6) for this issue conception is that it provides a more realistic account of voter policy preferences. In their view, voters do not discriminate between policy gradations as assumed by the proximity

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[1] The list of references includes eleven contributions with Rabinowitz, Macdonald, or Listhaug as the first author. All but one (Listhaug, Macdonald, and Rabinowitz 1994b) focus on the new theory.

[2] See, for example, Rabinowitz and Macdonald 1989, 93, the abstract; Macdonald, Listhaug, and Rabinowitz 1991, 1107, the abstract. Virtually all Rabinowitz, Macdonald, and Listhaug's writings on the new theory pit it against the classical model.

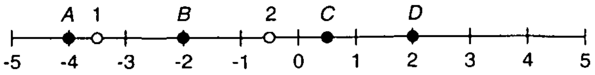
model but only possess a diffuse sense of direction coupled with a smaller or greater sense of urgency.

Research based on the proximity model has long given prominent attention to the hypothesis that the salience of an issue conditions its effect on the vote.[3] Hence, the point made by the directional conception is not simply that intensity matters. Rather, the new idea is that policy alternatives are always understood in binary terms, which leaves intensity as the *only continuum*. Thus, when voters are asked to indicate their issue stands on a continuous or semicontinous scale, the differences between responses falling on either half of the scale will express variations in intensity rather than variations in policy position. It follows that separate questions designed to measure issue salience are redundant, since the intensity component is already in hand.

An example will help clarify the implications of these ideas for the relationship between issue stands and party preference formation. Figure 1 shows the stands taken by two voters (1 and 2) and four parties (*A* to *D*) on an issue dimension. If we take the dimension to represent a policy continuum, party preferences will depend on how close the various parties are to the points representing voter ideals. If, by contrast, the dimension is assumed to represent two directions or sides (indicated by the sign of the score, with zero as a neutral point) combined with the intensity with which either side is supported (indicated by the absolute value of the score), party preference formation will follow somewhat different rules (see Rabinowitz and Macdonald 1989, 97–8). First, voters always have reason to prefer a party favoring their own side to one favoring the opposite side. I will refer to this as the *side rule*. Second, voters also have reason to prefer a more intense party on their own side to a less intense one since, under the directional model, the intensity of a party's stand is assumed to represent the probability that it will pursue the preferred policy alternative (Macdonald and Rabinowitz 1993a, 63). I will call this the *party-intensity rule*.

As shown in Figure 1, the extent to which the two rules discriminate depends on voter location. For voters far to the extremes, the preference orders predicted under the two issue conceptions are very similar. Beyond the last interparty midpoint, there is no distinction at all, as illustrated by voter 1. For voters in the middle ground, the predictions are more likely to diverge. Thus, if voter 2 uses the directional conception, *A* and *B* will be preferred to *C* and *D* according to the side rule, although *C* is closer than *B* and *D* closer than *A*. In addition, *A* will be preferred to *B* according to the party-intensity rule, although *B* is more proximate.

FIGURE 1. Illustration of Predictions Based on the Proximity and the Directional Model



Preference Order of	Proximity Model	Directional Model
Voter 1	*ABCD*	*ABCD*
Voter 2	*CBDA*	*ABCD*

Mathematically, both rules are embodied in a single principle stating that the utility any one voter associates with each of a set of parties increases monotonically with the product of voter and party issue stands. In a multidimensional space, the simple product is replaced by the scalar product, that is, the sum of the dimensionwise products. Thus, if we let rank-order(x_1, x_2, . . ., x_n) denote a vector containing the rank order of a series of quantities, x_1, x_2, . . ., x_n, the directional model proposes that for any one voter, *i*,

$$\text{rank-order}(U_{iA}, U_{iB}, \ldots, U_{iJ}) = \text{rank-order}(\mathbf{i}\cdot\mathbf{A}, \mathbf{i}\cdot\mathbf{B}, \ldots, \mathbf{i}\cdot\mathbf{J}), \qquad (1)$$

where U_{iA} is the utility that voter *i* associates with party *A* and $\mathbf{i}\cdot\mathbf{A}$ the scalar product of the issue stands taken by the same voter and party. The proximity model, by contrast, posits that

$$\text{rank-order}(U_{iA}, U_{iB}, \ldots, U_{iJ}) = \text{rank-order}(-d_{iA}, -d_{iB}, \ldots, -d_{iJ}), \qquad (2)$$

where $-d_{iA}$ is the proximity (i.e., distance inverted) of voter *i*'s issue stands to those of party *A*.

Implicit in the transition to the multidimensional case made in these formalizations is a third rule distinguishing the directional from the proximity model, namely, that the weight of a dimension in the decision calculus is directly proportional to the voter's deviation from its midpoint. This third distinctive feature of the directional model can be named the *voter-intensity rule*.[4]

The Region of Acceptability

Thus far, it would seem that the two models are structurally equivalent in that they both rest on a single, unifying principle: scalar product in the one case, proximity in the other. Such, however, is not the case. The directional model incorporates a second principle without counterpart in the proximity theory, namely, that of the *region of acceptability*.

This region is a portion of the issue space falling within a directionally invariant Euclidean distance, *r*, from the origin. In a unidimensional space it is thus the interval $-r$ to $+r$. In a two-dimensional space it is the area of a circle centered on the neutral point with radius r. In a space with three or more dimensions it is the interior of a sphere or hypersphere with the same attributes. All members of an electorate are assumed to have a common understanding of the size and location of the region, independently of their own locations.

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[3] Some studies have found support for this hypothesis, others have not. Krosnick (1988) provides a good review of past research in the area along with new tests and some methodological explanations for the failure to distinguish a salience effect in some of the prior attempts.

[4] It can be argued that the proximity model contains an implicit weighting scheme of a somewhat similar kind in that the variance in voter–party proximities on any one dimension tends to decline as the voter approaches the center of the party spectrum. Yet, the effect of this tendency is neither as strong nor as straightforward as that of the voter-intensity rule in the directional case.

For parties inside the region, utility is assumed to be a function of the scalar product alone. Parties outside its boundary, however, may incur a *penalty* that detracts from the utility derived on the basis of the scalar product. Equation 1 thus holds only in the special case in which the parties are all inside the region. More generally, the directional model posits that

$$\text{rank-order}(U_{iA}, U_{iB}, \ldots, U_{iJ}) = \text{rank-order}(\mathbf{i} \cdot \mathbf{A} - P_{iA}, \mathbf{i} \cdot \mathbf{B} - P_{iB}, \ldots, \mathbf{i} \cdot \mathbf{J} - P_{iJ}), \tag{3}$$

where P_{iA} is the penalty voter i imposes on party A, the value of which is zero if A is inside the region.

The rationale offered for the region of acceptability and the penalty varies somewhat. Rabinowitz and Macdonald (1989, 108) argue—in the context of U.S. politics—that a candidate "must convince voters of his or her reasonableness. Voters are wary of candidates who seem radical and project harshness or stridency. The label 'extremist' can attach to such candidates and severely hamper the enthusiasm of potential supporters." Macdonald, Listhaug, and Rabinowitz (1991, 1110) emphasize—in the context of Norwegian politics—the extent to which voters feel "the party can function effectively in government. A party that is viewed as overly intense or excessively strident on issues will be evaluated less favorably than a party regarded as responsible and more able to govern effectively."

Implications for Falsifiability

The size of the region of acceptability as well as the magnitude of the penalty for parties beyond its boundary are left largely unspecified by the theory.[5] No size constraints are imposed on the region itself. The only restrictions on the penalty are that parties inside the region will not be penalized and that a party located outside the region will always be electorally inferior to one placed on the boundary in the same direction (Rabinowitz and Macdonald 1989, 108, 117; Rabinowitz, Macdonald, and Listhaug 1991, 152).[6] Hence, for parties outside the region, the penalty can vary freely across parties as well as voters as long as it stays above the threshold set by the second of the two restrictions.

The unspecified nature of the region of acceptability and the penalty, absolutely as well as in relation to the scalar product, renders the directional theory considerably less falsifiable than the proximity theory. As shown by Popper (1980, chapter 7, Appendix *viii), the number of freely adjustable parameters in a theory is directly and negatively related to its degree of falsifiability.

The basic point can easily be illustrated. Consider again the case of voter 2 in Figure 1. The proximity model predicts that the preference order of this voter will be *CBDA*, whereas the directional model predicts *ABCD*. Suppose the preference order actually observed is *BCDA*. As far as the proximity model is concerned, our only option is to concede that the observed order is in partial disagreement with the predicted one. With respect to the directional model, by contrast, we still have some cards to play. The only modifications required in order to make the prediction fit the observation is to suggest that (1) the boundary of the region of acceptability falls at a distance of somewhere between two and four units from the origin and (2) the penalty imposed on party A by voter 2 exceeds three units in the currency of the scalar product. In fact, by suitably modifying the size of the region and the penalties, the theory can be made to comply with any preference order. In applications involving an entire electorate rather than a single voter, the possibilities in this regard are restricted by the requirement that the region be the same for all members of the electorate. Nevertheless, the degree of plasticity remains considerable (see Appendix C).

A Theoretical Summary

The two principles on which the directional model rests allow us to distinguish between two versions of the theory: a *reduced* version based on the scalar product alone and a *complete* version including the region of acceptability and the penalty. The reduced version resembles the proximity model in several important respects. First, both are rational-choice models in the sense that party preferences are assumed to be a rational response to the issue stands taken by voters and parties. Second, by implication, both are also proximity models in the sense that voters are assumed to act so as to maximize the agreement between the desired policies and those enacted. Finally, both possess a fairly high and equal degree of falsifiability.

The key difference lies in the conceptualization of issue dimensions. This difference, in turn, gives rise to a distinction between the spatial functions linking voter and party issue stands to voter utility. If issue stands are taken to represent policy positions, then party preferences will be a function of proximity. If they are taken to represent direction combined with intensity, then party preferences will instead be a function of the scalar product. The way in which the decision calculus embodied by the scalar product differs from that of proximity can be expressed by means of three rules: the side rule, the party-intensity rule, and the voter-intensity rule.

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[5] Examples and operationalizations are sometimes provided, but these are not presented as being part of, or fully implied by, the theory.

[6] This, in turn, implies that all voters, irrespective of their locations, will reject a party A outside the region, in favor of a party B on the boundary in the same direction. If A would be preferred by voters in some locations, there would be no guarantee that B would perform better than A in the electorate as a whole, since the theory makes no assumptions about the spatial distribution of voters. It should be observed that this partial specification of the penalty function in Rabinowitz, Macdonald, and Listhaug's first two articles on the topic is not repeated in later contributions, where they leave the shape of the function more open (see, e.g., Macdonald, Listhaug, and Rabinowitz 1991, 1113, 1127). To the extent that this can be understood as a rejection of the prior specification or a denial of its universal applicability, it implies a further reduction in the falsifiability of the directional model.

In the complete version of the directional theory, utility is no longer a function of the scalar product alone but also of the region of acceptability and the penalty. The weak specification of these additional constructs makes the complete version inferior to the proximity model in terms of falsifiability.

PREPARING THE TEST BED

The basic premise for the empirical analyses of the two models reported by Rabinowitz, Macdonald, and Listhaug is the measure of issue stands introduced in the U.S. national election studies in 1968. This question format presents the respondent with a scale whose endpoints are labeled with two policy alternatives. The remaining points are unlabeled. The respondent is thus free to interpret the scale in either a positional or a direction-intensity sense. By comparing the observed candidate or party preferences with those predicted under the two issue conceptions, one can infer the extent to which the conceptions are actually used by the voters and thereby determine the degree of support enjoyed by the two models.

While the analyses I will present match those of Rabinowitz, Macdonald, and Listhaug in this fundamental respect, they differ in some other vital regards. In the following subsections, I consider six choices facing us on the way toward a proper test of the two theories. In three of the six cases, I argue that the choice should be another than that made by Rabinowitz, Macdonald, and Listhaug. In the remaining three, I find it important to present my arguments for other reasons.

In later empirical sections, I first test the two theories in the fashion I consider most appropriate. As will become evident, this leads to an outcome diametrically opposed to that previously obtained. I then specify the extent to which this outcome is dependent on each of the three ways in which my methods differ from those of Rabinowitz, Macdonald, and Listhaug. Finally, I try to explain the effect of the most significant choice, that between intrapersonal and interpersonal comparisons.

Intrapersonal versus Interpersonal Comparisons

Suppose we ask two voters, 1 and 2, to express their likes and dislikes with respect to two parties, *A* and *B*. Suppose in addition that our instrument is the thermometer scale commonly used in survey research, with the maximum score of 100 indicating a very positive evaluation and the minimum score of 0 a very negative evaluation. Let us finally assume that our results are those displayed in Table 1.

TABLE 1. Illustration of Intrapersonal versus Interpersonal Comparisons of Evaluation Scores

	Party *A*	Party *B*
Voter 1	70	60
Voter 2	40	50

The table basically allows two kinds of comparison. By comparing across columns for a given row we can investigate how the same voter relates to different parties. By comparing across rows for a given column we can investigate how different voters relate to the same party. The first is an intrapersonal comparison, the second an interpersonal one.

It is not difficult to see which of the two comparisons we should make in order to test our two theories. Both are theories of individual choice. They thus predict the utility any *one* person will associate with each alternative *relative to other alternatives* and thereby the preference order with respect to the whole set of alternatives (see equations 1–3). They say nothing whatsoever about how the utility of a certain alternative to one person compares with its utility to another person. The reason is that interpersonal comparisons of utility are completely irrelevant to individual choice.[7] Voter 2 gives party *B* a lower rating than does voter 1. Nevertheless, *B* is the preferred party of voter 2 but not of voter 1. What voter 1 thinks about the parties is of no consequence to the decision of voter 2 and vice versa. Only the intrapersonal comparison matters.

In view of this, it is highly remarkable that none of the seven contributions by Rabinowitz, Macdonald, and Listhaug that deliver the primary individual-level evidence in support of the directional model over the proximity model contains a single empirical test of the proper kind. All analyses are based on interpersonal rather than intrapersonal comparisons.[8]

In the prototypical case, the evaluation scores for each party or candidate are analyzed separately, so that all the variation is contained between rather than within individuals. In some cases, pooled analyses combining evaluation scores for multiple parties or

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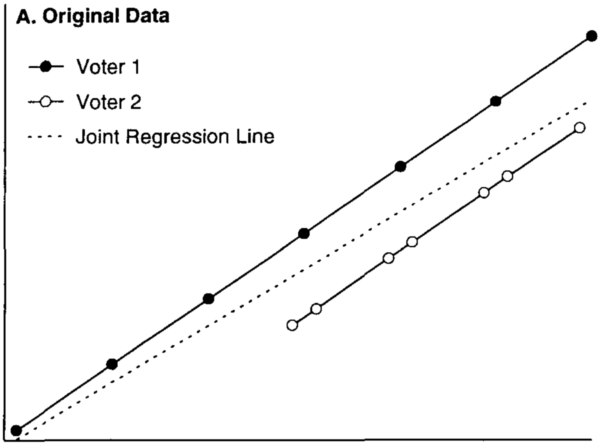
[7] As a rule, interpersonal comparisons of utility are completely irrelevant to *positive* choice theory. As pointed out by Elster and Roemer (1991), there are cases in which the observers must take into account that the observed individuals sometimes make such comparisons, as in the study of envy and of bargaining. Yet, even in these special cases—none of which is relevant here—there is no need for the observers themselves to compare utilities across individuals. In *normative* choice theory, by contrast, it is often desirable to make such comparisons but difficult to find a justifiable way of doing so. For a modern discussion of this problem, see the contributions to the volume of which Elster and Roemer 1991 is part.

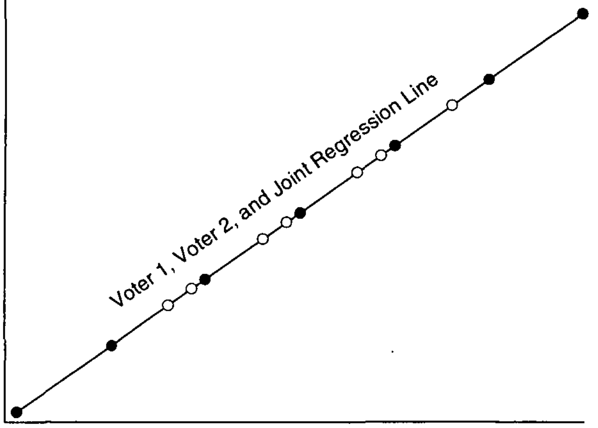
[8] The seven contributions I have in mind are: Rabinowitz and Macdonald 1989; Macdonald, Listhaug, and Rabinowitz 1991; Macdonald and Rabinowitz 1993b; Rabinowitz, Macdonald, and Listhaug 1993; Listhaug, Macdonald, and Rabinowitz 1994a; Macdonald, Rabinowitz, and Listhaug 1995a and b. In an eighth article—Rabinowitz, Macdonald, and Listhaug 1991—which in general is less ambitious than any of the aforementioned when it comes to individual-level testing one does find a few analyses based on intrapersonal comparisons. These analyses are discussed in Appendix C.

FIGURE 2. Distilling the Intrapersonal Component

A. Original Data



B. Deviations from Individual Means



candidates are also reported. Yet, since no attempt is made to isolate the intrapersonal element in these analyses, they become for all practical purposes just another interpersonal comparison.

In terms of the example presented in Table 1, the type of analysis performed by Rabinowitz, Macdonald, and Listhaug thus amounts to an explanation of why voter 1 provides a higher evaluation of party *A* as well as party *B* than does voter 2. What the two theories purport to explain, however, is why voter 1 prefers *A* to *B* and why the opposite holds for voter 2. The bulk of empirical work on the relative performance of the two theories thus tests them on predictions that neither makes. It follows that the results can neither refute nor corroborate either theory.

How, then, can we perform a test of the proper kind? Equations 1–3 above suggest an immediate solution. The theories predict that the rank order of the dependent variable *within* individuals should match the corresponding rank order of the independent one. Hence, all we have to do in order to assess the fit is to transform the data to intrapersonal ranks and then compute the correlation between them (Spearman's rho applied intrapersonally).

While this solution has the advantage of following directly from theoretical specifications without any additional assumptions, it does not readily extend to the multivariate case. Hence, it is not well suited to tests of the complete version of the directional model, where the lack of theoretical precision makes it necessary to treat the penalty factor associated with the region of acceptability as a separate independent variable. Furthermore, it cannot provide a precise estimate of the relative empirical strength of the two theories, particularly since the scalar-product and proximity measures by definition are strongly intercorrelated. The obvious solution to this problem is to switch to interval-level methods (as do Rabinowitz, Macdonald, and Listhaug in their analyses). This, in turn, calls for a somewhat more specific assumption of functional form (e.g., linear) than that required by the theories (monotonically increasing).

Just as in the ordinal case, however, we must make sure that the variation analyzed is theoretically relevant, that is, intrapersonal rather than interpersonal. To get a better grasp of this problem as well as its solution, consider the two diagrams displayed in Figure 2, which illustrate the relationship between a dependent variable, such as party evaluation, and a predictor, such as proximity or scalar product. Figure 2A shows the intrapersonal relationship for two different voters. In both cases, the relationship is perfect. Once the independent variable is held constant, however, voter 1 provides more positive evaluations than does voter 2. Due to this interpersonal difference, which is irrelevant with respect to voter choice, the regression line for the two voters combined shows a less than ideal fit. Hence, it does not correctly reflect the perfect correspondence between theory and data.

The problem can be resolved by subtracting the individual mean value from the scores of each respondent. This purges the data of the interpersonal variance and leaves only the intrapersonal component. As shown in Figure 2B, the joint regression line now overlaps completely with those of the two voters, thereby mirroring the strength of the intrapersonal relationships we want to assess.

Formally, the operation described corresponds to the following regression equation, which models the dependent variable as an intrapersonal function of the predictor:

$$Y_{ij} - \bar{Y}_i = b(X_{ij} - \bar{X}_i) + u_{ij}. \qquad (4)$$

The symbol Y_{ij} denotes voter *i*'s evaluation of party *j*, $\bar{Y}_i$ voter *i*'s mean evaluation across all parties, X_{ij} the score of voter *i* for party *j* on the independent variable whose effect we want to assess (e.g., proximity or scalar product), and $\bar{X}_i$ the mean for voter *i* across all parties on the same variable. Of course, multiple predictors can be inserted in the same fashion. Note that no intercept term is required. If added, its estimated value is always nil since both $Y_{ij} - \bar{Y}_i$ and $X_{ij} - \bar{X}_i$ have a mean of zero.

Evaluation Scores versus the Vote

As already mentioned, evaluation scores have served as the dependent variable in most prior tests of the relative performance of the two theories. In view of the fact that both theories claim to be theories of voting, one is prompted to ask: Why not the actual vote?

My answer, which differs from that of Rabinowitz, Macdonald, and Listhaug although we both prefer evaluation scores,[9] is based on theoretical as well as methodological considerations. Both theories predict the entire preference order of each voter, not just the most preferred alternative (see equations 1–3). Evaluation scores allow a complete test of these predictions, whereas the vote limits us to the peak preference. Evaluation scores also provide information about the size of the utility differences, which is useful if, on methodological grounds, we would like to proceed on the basis of interval-level assumptions. The vote, by contrast, contains no information of this kind. Consequently, while both measures can and will be used to test the two theories, evaluation scores permit a more comprehensive, precise, and robust assessment.[10]

My considerations on this score accord with those motivating the use of evaluations as a mediator between the vote and the factors of substantive interest (e.g., issue stands) in general models of voting (e.g., Markus and Converse 1979, Page and Jones 1979). In view of the superior measurement characteristics of evaluation scores and the efficiency with which they are long since known to predict the vote (Brody and Page 1973), there seems to be much to gain and little to lose from such a strategy.

Individual versus Aggregate Images of Party Issue Stands

To construct the independent variables for the models, two types of information are required. Not only do we need to know each voter's own views on the issues but also his or her beliefs as to where the parties are located. Although voters may at times be mistaken about these locations, it is their personal beliefs, whether right or wrong, that will guide preference formation. The best source for both types of information is therefore the individual voter.

This conclusion is not uncontroversial. Instead of using the information on party locations held by each voter, as I propose, Rabinowitz, Macdonald, and Listhaug base their analyses on the mean party placements for the entire aggregate of voters. Their main arguments in support of this approach are twofold (Rabinowitz and Macdonald 1989, 119, n7; Macdonald, Listhaug, and Rabinowitz 1991, 1130, n12; Macdonald, Rabinowitz, and Listhaug 1995b, 460). First, both theories require each party to have a single location on each issue. If not, it becomes impossible to link party strategy with popular support. Second, the images of party issue stands held by individual voters are subject to projection effects (wishful thinking) based on the voters' own issue stands and their party evaluations. Using such images as part of the independent variable therefore entails a risk of reverse causality.

While both statements are correct, they are irrelevant as arguments in the case at issue. As for the first argument, both theories do require that we, as scientific observers, are willing to accept the notion of objectively determinable party locations for the purpose of assessing party strategy. Neither theory demands, however, that all voters have identical and perfectly accurate information about those locations. If this were indeed a requirement, we could immediately write off both theories as empirically unreasonable. To maintain the link between electoral choice and party strategy, it is sufficient to assume that the issue stands actually taken by parties have some effect on the images of those stands held by voters. This, of course, is a much more realistic assumption.[11]

The position taken by Rabinowitz, Macdonald, and Listhaug on this matter is particularly difficult to understand in view of the cognitive shortcomings they ascribe to ordinary voters as well as their claims to having developed a model more attuned to these shortcomings (see, e.g., Rabinowitz and Macdonald 1989, 94–5). It is not entirely convincing to argue that a good model of electoral choice should be based on the presumption that the "vast preponderance of citizens operate with low levels of information" and that their policy preferences do not "go beyond a diffuse sense of direction" but also on the assumption that they are nevertheless perfectly aware of where parties or candidates are located.

As for the second argument, the presence of projection effects certainly constitutes a problem if the principal task is to estimate the *absolute* degree to which either theory is able to explain variations in the dependent variable. This, however, is not the main objective in the case at issue. The foremost concern is the performance of the two theories *relative* to each other.

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[9] In Rabinowitz, Macdonald, and Listhaug's early contributions, the question is never raised. In some of their later pieces, however, it is explicitly addressed (Macdonald and Rabinowitz 1993b, 63; Macdonald, Rabinowitz, and Listhaug 1995b, 475–6). The answer provided is that (1) vote choice is based on the utility difference an individual sees between two parties and (2) the two theories are, under some provisions, indistinguishable through interpersonal comparisons of such differences. For reasons already discussed, however, a test of the two theories should be based on intrapersonal comparisons, in which case the theories remain distinguishable irrespective of which measure we use. The choice between the two measures must therefore be made on other grounds.

[10] Some further specifications of the reasons favoring evaluation scores over the actual vote are given in the subsection "Choosing the Testing Ground" below. See also Appendix C for an illustration. Note, finally, that the loss of information incurred by taking the actual vote as the dependent variable is likely to prompt simplification of the independent variables as well. For reasons spelled out in the last two sentences of note 14, the ensuing loss of precision in the predictors increases the risk of faulty inferences in simultaneous tests of the two theories.

[11] The accuracy of voter images of party or candidate issue stands has been studied by, for example, Granberg and Holmberg (1988, chapter 3), Powell (1989), and Listhaug, Macdonald, and Rabinowitz (1994b). A general conclusion from these studies is that voter images are far from perfectly accurate or unanimous but nevertheless are clearly related to what can realistically be considered the objectively true locations.

Given this purpose, the potential presence of projection processes constitutes no problem. The reason is simple. Projection takes place because people sometimes find it easier to alter their cognition to fit their evaluation rather than the other way around. Regardless of the causal direction, however, the notion of *fit* should be the same. There is no psychological reason to expect a directional voter to project on the basis of proximity inasmuch as this would not make his or her mental state more balanced. On the contrary, projection for such a voter should be based on scalar product and penalty. For a proximity voter, we would of course expect the opposite, that is, projection based on proximity but not on scalar product and penalty. It follows that the unwarranted increase in statistical association due to projection should affect the results for the two theories in direct proportion to their true explanatory power. Hence, their estimated strength relative to each other should be the same regardless of whether projection is present or not.[12]

Consequently, individual party placements do not, in this context, possess the drawbacks with which they have been charged. Mean party placements, by contrast, give rise to several problems, the exact nature of which depends on our starting point.

Suppose, on the one hand, that we would like to proceed on the assumption that voters have perfect information about party issue stands. The target of our operationalization would then be the true party locations. The mean placements, however, are not a particularly good measure of these locations. Although they are likely to mirror some properties of the true account, such as the order of the parties with respect to each other, they are apt to be more or less severely distorted in other respects. For example, Powell (1989) demonstrates a bias toward the middle of the scale due to the fact that the party placement data partly consist of random guesses. This bias, furthermore, can be shown to have more serious consequences for the proximity than for the directional model.[13]

Suppose, on the other hand, that we wish to pursue what I consider to be the most fruitful objective, namely, to assess how voters actually think rather than how they might think if they all had perfect information. Our target would then be the actual cognitive map of each voter. The contorted pictures of those maps that result if we use mean rather than individual party placements cause two different types of problems.

First, we have no guarantees that the distortions will affect a test of the two theories in a fair and unbiased manner. On the contrary, there are several reasons to think that will not be the case.[14] Second, and equally serious, we risk losing sight of a perceptual process that may well be of vital importance for our understanding of the interaction between party systems and electorates. If we are seriously interested in that interaction, then we need to study the perceptual process, not hide it through aggregation or assume it away.

In his now classical comment on the Downsian theory, Stokes (1963, 376) concluded that insight into the applicability of that model (and others of its kind) would best be furthered by treating "as explicit variables the cognitive phenomena that the prevailing model removes from the discussion by assumption." In my view, there are many good reasons for, and few against, following his advice.[15]

Specifying the Concept of Proximity

I have hitherto spoken rather indifferently about distance and its opposite, proximity, as though the meaning of these concepts were self-evident. As long as the space under consideration is unidimensional, such is indeed the case. In a multidimensional space, by contrast, many notions of distance are conceivable. To my knowledge, however, most prior work on spatial choice models restricts itself to two varieties, Euclidean distance and city-block distance, and I find no compelling reasons to proceed beyond these alternatives here. Formally, the city-block distance between two points representing a voter, i, and a party, j, in an n-dimensional space can be written as

$$\sum_{k=1}^{n} |i_k - j_k|,$$

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[12] The same kind of reasoning applies to persuasion effects, that is, the effect of party locations on the voters' own issue stands. Note, in addition, that the dynamic estimates of Markus and Converse (1979, 1061) show both effects to exist but neither to be of overwhelming magnitude. This result is echoed in an analysis by Merrill (1995, 284–5), whose findings additionally indicate that proximity-driven projection does not threaten the validity of comparative tests of the two theories based on individual party placements.

[13] With the midpoint scored 0, the bias due to guessing can be described by the function $x_j = (1 - p)X_j$, where x_j is the estimated location of party j, X_j the true location of the same party, and p the proportion of guesses. It follows that the estimated scalar product for all party–voter combinations will equal the true scalar product multiplied by $(1 - p)$. This in turn implies that the predictions made on the basis of the estimated scalar products will be the same as those based on the true scalar products. The estimated proximities, however, will not be a simple linear transformation of the true proximities, and the predictions made on the basis of the former will as a rule be different than those based on the latter.

[14] Prior research (e.g., Aldrich and McKelvey 1977) indicates that all respondents do not interpret the end-points and units of response scales in the same fashion. Mean party placements do not take proper account of the ensuing variations in respondent scale use. For reasons similar to those discussed in the previous note, this is likely to have more serious consequences for the proximity than for the directional model. A second problem is that the mean-placement method allows parties to be located anywhere on the scale, whereas voters are forced into discrete slots. This may lead to severe consequences in tests of the proximity model. For instance, a party may obtain a mean that makes it theoretically impossible for any voter to take a stand so as to make the party in question the closest one (see Appendix C for an illustration). Finally, since proximity is by definition strongly correlated with the scalar product, it is generally of great importance to keep unreliability in the predictors at a minimum in simultaneous tests of the two models. There is otherwise a considerable risk that one theory will be credited with effects actually attributable to the other (cf. Blalock 1979, 477; Westholm 1987).

[15] Others have arrived at similar conclusions, although their arguments are not necessarily the same as mine. See, for example, Gilljam 1997; Merrill 1993, 1994, 1995; Pierce 1997.

that is, the simple sum of the dimensionwise distances, whereas the formula for Euclidean distance is

$$\sqrt{\sum_{k=1}^{n} (i_k - j_k)^2},$$

that is, the square root of the sum of the squared dimensionwise distances.

Neither the proximity theory itself nor the general concept of rationality requires or favors either definition. In the absence of such theoretical guidelines, I shall argue that there are three reasons to prefer the city-block conception. First, as Ordeshook (1986, 22–3) puts it, the city-block metric "nicely summarize[s] the intuition that, when comparing candidates, voters look separately at how different each candidate is from their most preferred position on each election issue. They then vote for the candidate whose total distance across all issues is the least." According to the Euclidean conception, voter information processing would follow a more complex logic.

Second, the city-block metric makes the transition from the unidimensional to the multidimensional case mathematically parallel for the two theories. In the unidimensional case, the directional model is based on the product of the voter's and the party's score, the proximity model on the unsigned difference between them. Given a city-block conception of proximity, the multidimensional versions are both based on the simple sum of the respective unidimensional quantities.

Third, the city-block conception offers methodological advantages. City-block distances present no difficulties in moving from a constrained proximity model, that is, one in which the relative weights of the dimensions are predetermined, to an unconstrained model, that is, one in which the weights are left free for estimation. Under the Euclidean conception, by contrast, the unconstrained case yields a regression equation which is nonlinear in the parameters. While such equations can still be estimated by resorting to special techniques, it is prudent to avoid them unless there are strong reasons to the contrary. Of course, the problem may also be resolved by modeling utility as a quadratic rather than linear function of distance, but this is appropriate only if the quadratic form can be shown to fit the data at least as well as the linear.

In their tests of the proximity model, Rabinowitz, Macdonald, and Listhaug switch back and forth between different distance conceptions and functional forms. In some cases they model utility as a function of Euclidean distance, in others as a function of squared Euclidean distance (e.g., to accommodate the demands of their so-called mixed model), and in still others as a function of city-block distance (when estimating unconstrained models). For obvious reasons, it is desirable to avoid such *ad hoc* shifts between specifications. In the present study, the city-block specification is used for all analyses of the proximity model except those whose purpose is to assess the effect of differences in method.

Specifying the Region of Acceptability and the Penalty

To test the complete version of the directional model, we must operationalize the region of acceptability and the associated penalty. One difficulty in accomplishing this task is that the theory provides no criterion whereby we can decide the size of the region. Another problem is that the size of the penalty for a party outside the region is left largely unspecified by the theory.

Uncertainty is further increased by the absence of justifications for the decisions taken when initially designing these constructs. One of the most remarkable features of the region of acceptability is that it is defined as a property of the entire electorate rather than as an attribute of the individual voter. This immediately prompts several questions. Why, for example, should an extremist voter see the same region as a voter located in the center or at the opposite extreme? And why should we expect such a voter to penalize a party sharing his or her own direction and degree of extremism? The region of acceptability is, as it were, a region in search of its acceptor.[16]

While the aggregate nature of the region of acceptability remains a puzzle, a partial response is offered by Rabinowitz, Macdonald, and Listhaug (1991, 152), who introduce the idea that the size of the penalty may vary with voter location although the region itself stays constant. More specifically, they suggest that "voters who are themselves intense and sympathetic to [a party outside the region] might apply no penalty or a very small one, while other voters might apply a larger penalty." They also present a class of mathematical functions corresponding to this idea but do not attempt to apply it empirically. Regrettably, the formalization they provide is inconsistent with their verbal suggestion as well as with other requirements of the directional theory. As shown in Appendix A, the inconsistency is rooted in a contradiction between the requirements themselves and therefore not so easily remedied. Hence, the theoretical line of development proposed by Rabinowitz, Macdonald, and Listhaug (1991) cannot serve as a guideline for the operationalization of the penalty.

In view of this situation, I have chosen to take my point of departure in operationalizations actually used by Rabinowitz, Macdonald, and Listhaug in empirical application. This, of course, does not resolve the problem of justifying these particular operationalizations in comparison with other alternatives equally compatible with the theory. Nor does it imply that I will refrain from modifying them when necessary in order to meet theoretical or methodological requirements. Nevertheless, they make it possible empirically to apply the region of acceptability and the penalty, and to do so in a manner which Rabinowitz, Macdonald, and Listhaug themselves have previously found acceptable.

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[16] An obvious solution to this problem would be to accord each individual a private region of acceptability. Once this step is taken, however, the distinction between the two theories becomes very tenuous. A directional model thus modified is more or less tantamount to a hybrid model in which utility is partly determined by party–voter distance and partly by the scalar product (cf. Iversen 1994a and b; Merrill and Grofman 1997).

Choosing the Testing Ground

The two theories have already been tested on a considerable number of data sets. It is not possible, of course, to offer a reanalysis of all these data within the confines of a single article. Nor is such an extensive task required in order to refute the claims made by Rabinowitz, Macdonald, and Listhaug, since for reasons already discussed their prior analyses have failed to test the theories on the predictions they actually make.

It remains, however, to find a good starting point. In my view, the best alternative is that through which Macdonald, Listhaug, and Rabinowitz (1991) introduce the directional model as a general theory of electoral choice in multiparty systems: the Norwegian 1989 election study.[17] First, this study meets two basic requirements: a rich set of items assessing respondent as well as party issue locations and a prior operationalization of the region of acceptability and the penalty based on these items. Second, it constitutes a more representative case than other conceivable alternatives, such as the United States and Britain. As Rabinowitz, Macdonald, and Listhaug (1991, 148) put it: "Most of the world's democracies are multiparty proportional systems. If basic models of voting are to have broad relevance for the understanding of democratic politics, they must be applicable to multiparty systems."

In some instances, the quest for a representative case runs counter to the quest for a crucial case. Not so with respect to Norway, where the relatively large number of parties distinctly improves our ability to discriminate between the two theories. Recall the side rule and the party-intensity rule, both of which help us tell the two theories apart. Certain conditions must be fulfilled in order for them to do so, however. The side rule discriminates only when a party on the voter's side is more distant than a party on the opposite side. The party-intensity rule yields different predictions only when there are at least two parties on the voter's side, one of which is more extreme than that to which the voter is closest. Both of these conditions are likely to occur with much greater frequency in a system with many parties than in one with only two or three contenders.

A final reason for focusing on a multiparty system such as the Norwegian is that it offers a more robust test. The relatively long preference orders that such a system puts at our disposal make the test less sensitive to arbitrary decisions regarding the operationalization of the region of acceptability and the penalty. In addition, they reduce the risk that factors outside the investigated issue space, which might favor some contenders over others, will seriously distort the outcome of the test. Note, however, that these advantages are contingent on the use of evaluation scores (full preference order) rather than actual vote (partial preference order) as the dependent variable. Only then is the greater robustness of the test fully realized (see Appendix C).

PROXIMITY REHABILITATED

Seven major parties competed for seats in the 1989 Storting elections: the Socialist Left Party, the Labor Party, the Liberals, the Center Party, the Christian Liberal Party, the Conservatives, and the Progressive Party. The proximity theory predicts that voter preferences across these parties will be based on party–voter distances. The directional theory predicts that they will instead be based on the respective scalar products minus any penalty for parties outside the region of acceptability.

In the subsection on intrapersonal versus interpersonal comparisons, I described two basic ways to test these predictions: an ordinal-level method, which follows directly from theoretical specifications but only allows a partial analysis, and an interval-level method, which requires auxiliary assumptions of functional form but permits a more complete analysis. Both methods will be applied in turn.

According to the ordinal-level method, we assess the predictions through the correlations between the intrapersonal rank orders of the variables involved (Spearman's rho applied intrapersonally). In the present case, the dependent variable, party evaluation, is based on thermometer ratings. The independent variables, *Proximity* and *Scalar Product*, are based on six issue items plus placement on a left–right scale.[18] Following the practice of Rabinowitz, Macdonald, and Listhaug, the seven items are weighted equally.

The prior results obtained by Macdonald, Listhaug, and Rabinowitz (1991) from the same data set would lead us to expect greater predictive power for the scalar product than for proximity.[19] The present test, however, shows the opposite. The intrapersonal rank-order correlation between party evaluation and proximity is

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[17] The study, conducted by Henry Valen and Bernt Aardal, is based on a nationally representative sample of all members of the electorate aged 18–80. Data were successfully collected, predominantly by means of personal interviews, from 2,195 respondents, which corresponds to a response rate of 73.2%. The data set is available from the Norwegian Social Science Data Services (NSD).

[18] The six issue items are: agricultural policy (maintain versus reduce government support for agriculture), environmental policy (give versus do not give priority to environmental protection over standard of living), immigration policy (more liberal versus more restrictive), health care policy (private versus public health care), alcohol policy (more liberal versus more restrictive), and crime policy (milder versus tougher punishment of violent crime). Responses to all issue items as well as left–right placement were obtained on a ten-point scale, originally scored 1–10 but rescored to range from −4.5 to 4.5 in all analyses presented here. Proximity scores were computed as $-\Sigma|i_k - j_k|/n_{ij}$ and scalar product scores as $\Sigma i_k j_k/n_{ij}$, where i_k is the score of voter i on item k, j_k the score of party j on the same issue, and n_{ij} the number of items with valid data for voter i and party j. Thermometer ratings were obtained on a 0–100 scale. An English translation of the question wording for all items used is provided by Macdonald, Listhaug, and Rabinowitz (1991, 1127–8, 1130, n10).

[19] According to Macdonald, Listhaug, and Rabinowitz (1991, 1121, Table 7, note *b*), the directional model outperforms the proximity model even before the penalty measure is taken into account.

.55, whereas that between party evaluation and scalar product amounts to .49, the difference between the two coefficients being statistically significant at the .001 level.[20]

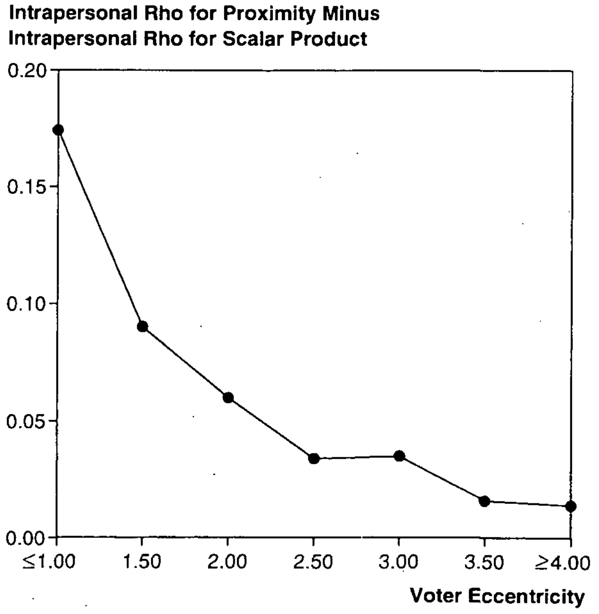
While the absolute magnitude of the difference is relatively small, this does not imply that it is of little theoretical significance. As previously mentioned, the proximity and scalar product measures are by definition strongly intercorrelated (.76 for the intrapersonal rank orders in the present case). Thus, even if one of the models had no explanatory power of its own, its spatial function would be nearly as strongly related to the dependent variable as that of the model actually accounting for the variation.

The high correlation between the proximity and the scalar-product measure is a reflection of the fact that the two models make similar, or even identical, predictions in many cases. Yet, the degree of similarity depends on voter location. Recall that two of the three prediction rules distinguishing the reduced version of the directional model from the proximity model—the side rule and the party-intensity rule—are more likely to result in different predictions for voters in the middle ground than for those at the extremes. To the extent that these two rules account for the observed difference in performance, we would therefore expect the gap between the two theories to vary with the voter's distance from the center.

That such is actually the case is shown in Figure 3, which relates the predictive advantage of the proximity model (i.e., the intrapersonal rho for proximity minus the intrapersonal rho for the scalar product) to the degree of voter eccentricity (i.e., the extent to which the voter is off center).[21] For voters taking very eccentric stands, the difference between the models is nearly nil. From there, the advantage of the proximity model increases monotonically to a value just below .20 for voters located close to the center.

The results hitherto presented permit the conclusion that the proximity model outperforms the reduced version of the directional model, but we have yet to consider the region of acceptability and the penalty. By means of these constructs, the directional model may still be able to show its strength, although a potential improvement in its compatibility with the evidence is bought at the expense of a reduced degree of falsifiability.

FIGURE 3. Predictive Advantage of the Proximity Model as a Function of Voter Eccentricity



The multivariate framework required by the penalty factor mandates a switch to interval-level methods. This simultaneously allows us to determine the net explanatory power of one theory after controlling for that of the other. Table 2 presents the results of modeling party evaluation as a linear intrapersonal function (see equation 4) of proximity, scalar product, and penalty.[22] The operationalization of the penalty factor follows that of Macdonald, Listhaug, and Rabinowitz (1991) save for modifications due to the use of individual rather than mean party placements. This means that the variable is scored zero for parties placed inside the region and by a value corresponding to the Euclidean distance from the boundary for parties placed outside the region.[23]

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[20] The number of cases is 13,503 and the number of respondents 1,929. The test of significance used is described in Blalock 1979, 424–5, and its applicability justified by the observation that Spearman's rho is defined as the product-moment correlation applied to ranks. The significance level reported obtains even under the conservative assumption that N equals 1,929 rather than 13,503 minus 1,929.

[21] Eccentricity is operationalized, here as well as in subsequent analyses, as the respondent's average deviation from item midpoints, that is, as $\Sigma|i_k|/n_i$, where i_k is the score of voter i on item k, and n_i is the number of items with valid data for voter i. Alternative definitions of the central point on each dimension are, of course, conceivable; for example, the population mean or the location of the median party. In the case at issue, however, the practical difference between these alternative specifications is very small. The eccentricity values used in the figure have been rounded to the closest half unit.

[22] Rabinowitz, Macdonald, and Listhaug argue that a special statistical model, which they call a mixed model, should be used when estimating the relative effect of proximity and scalar product within the framework of a single, multivariate, statistical model. A description of this model is given in Appendix B. As shown in the same appendix, however, it offers no advantages but several disadvantages compared to a more conventional approach. There is thus no reason to depart from the latter. Furthermore, Macdonald, Listhaug, and Rabinowitz (1991) report results for statistical models including as well as excluding controls for factors outside the scope of the two choice theories, for example, social class and sex. The controls leave the outcome of their theory comparison substantively unaffected. In view of space constraints and in the absence of compelling empirical or theoretical reasons to the contrary, I focus solely on the variables singled out by the two theories.

[23] The region of acceptability itself is operationalized as being the same for all voters since this is a requirement of the directional theory. Party placements are allowed to vary across respondents, however, just as in the construction of the proximity and scalar-product measures (see the subsection "Individual versus Aggregate Images of Party Issue Stands"). Macdonald, Listhaug, and Rabinowitz (1991, 1130, n15) draw the border of the region at a Euclidean distance from the origin of 4.305 based on six items. Expressed in the

Table 2. Party Evaluation as an Intrapersonal Function of Proximity, Scalar Product, and Penalty

Independent Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Proximity	.62 (.007)	—	.60 (.015)	.63 (.008)	—	.63 (.025)
Scalar product	—	.55 (.008)	.03 (.015)	—	.53 (.007)	.00 (.022)
Penalty	—	—	—	.02 (.008)	−.23 (.007)	.02 (.012)
Adjusted R^2	.387	.301	.387	.387	.353	.387

Note: Entries for independent variables are standardized regression coefficients with standard errors in parentheses. Since the independent variables are not measured in the same units, the standardized regression coefficients provide the most meaningful way of comparing their effect. The standard errors reported here as well as in subsequent intrapersonal analyses take proper account of the fact that one degree of freedom per respondent is lost by subtracting the individual means. The number of cases is 13,496, and the number of respondents is 1,928. The analyses include only respondents having valid scores for all four variables across all seven parties.

Table 2 contains estimates for six statistical models. The first two are bivariate regressions with proximity and scalar product, respectively, as the only predictor. They thus constitute the interval-level equivalents of the ordinal-level analyses already performed. As expected under the assumption that a linear function is a reasonable approximation, the standardized regression coefficients (which in the bivariate case are identical to product-moment correlations) are both slightly higher than their ordinal-level counterparts, and the difference between them is slightly larger.

Model 3 includes both the proximity and the scalar-product measure. As a result, the effect attributable to the directional model drops to near zero, whereas that accounted for by the proximity model remains almost as high as in the bivariate case. Despite the high correlation between the independent variables (.87), the standard errors indicate that the coefficient estimates are quite precise. The relative strength of the two predictors is therefore by no means coincidental. Note also that adding the scalar product to a pure proximity model yields no perceptible growth in the proportion of variance explained, whereas adding proximity to a pure scalar-product model yields a substantial increase in predictive power (up by .086).

The three remaining regressions test the impact of the penalty. When combined with proximity alone, the penalty effect is close to zero, exactly as we would expect. When combined with the scalar product alone, it shows a substantively significant negative effect, which is also in line with prior expectations. The proportion of variance explained grows perceptibly in comparison with the pure scalar-product model (up by .052) but still trails that of the pure proximity model. Note that this is a generous test of the directional model in that it does not enforce the requirement that the predicted utility for a party outside the region of acceptability always be lower than for a party located on the boundary in the same direction. With this constraint enforced, the predictive power is lower.[24]

The sixth and final regression includes all three predictors. The results are essentially the same as for model 3. The coefficients for the scalar product and the penalty both drop to near zero, the coefficient for proximity remains on the same level as in the bivariate case, and there is no visible gain in predictive power compared to a pure proximity model. Thus, the proximity theory receives much stronger empirical support even when confronted with the complete version of the directional model.[25]

Although, for reasons previously explained, actual vote choice offers a less comprehensive, less precise, and less robust test of the two theories than the full range of evaluation scores, the results in this case generalize perfectly. The proximity model correctly predicts the vote in 52.5% of the cases as against 14.3% for a random model. The results for the directional model are 36.9% based on the scalar product alone and 49.7% with the penalty factor included.[26]

EXPLAINING THE OUTCOME

One question still remains. Why, exactly, are the results obtained here diametrically opposed to those previously reached by Macdonald, Listhaug, and Rabinowitz (1991) on the basis of the same data? Obviously, the answer must be sought in differences of method. Three

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currency of the normalized distance measure (distance relative to the number of dimensions) used in the present analysis, the value becomes $\sqrt{4.305^2/6} \approx 1.758$. The score of the penalty factor is $\sqrt{\Sigma j_k^2/n_j}$ -1.758 if the value is positive (i.e., if the party is placed outside the region) and zero if not. The symbol j_k denotes the score of party j on item k and n_j the number of items with valid data for party j. According to Macdonald, Listhaug, and Rabinowitz (1991, 1130, n14), one of the seven items used in constructing the proximity and scalar-product measure (crime policy) should not be included when defining the boundary of the region and the penalty factor since it is shown in their tests to have little effect on party evaluation. I have chosen to follow their practice in order to avoid a redefinition of the radius of the region.

[24] See footnote 6. In formal terms, the condition can be written as $-b_2 \geq b_1 \max(|\mathbf{i}|)$, where b_1 and b_2 are the unstandardized regression coefficients for the scalar-product and the penalty variable, respectively, and $\max(|\mathbf{i}|)$ the theoretical maximum length of any voter (or party) vector in the issue space under consideration. In this case, $-b_2 = 7.58$ and $b_1 \max(|\mathbf{i}|) = 2.99 \cdot 4.50 = 13.46$, which violates *the condition. Constraining the parameter estimates so that the* condition is met lowers the adjusted R^2 to .325.

[25] Models in which the weight of each issue was set free for estimation were also analyzed (cf. Macdonald, Listhaug, and Rabinowitz 1991, 1120–1). The results continue to support the proximity model over both versions of the directional model.

[26] *For the proximity model and the directional model without* penalty, the predicted party is the one having the highest proximity and scalar-product score, respectively. For the directional model with penalty, it is the party with the highest predicted score according to the estimates for the fifth model in Table 2. If several parties shared the top score, a share of the case was attributed to each of the tied parties. The analyses include only those respondents (numbering 1,674) who (1) reported voting for one of the seven parties for which there is party placement data, (2) actually took part in the election (according to public records), and (3) had valid scores for proximity, scalar product, and penalty across all seven parties.

TABLE 3. The Effect of Method Choices on Tests of the Proximity and the Directional Model

Type of Comparison and Independent Variable	Individual Party Placements			Mean Party Placements		
	City-Block Distance	Euclidean Distance	Euclidean Distance^2	City-Block Distance	Euclidean Distance	Euclidean Distance^2
Intrapersonal						
−Distance or −distance^2	.60 (.015)	.56 (.015)	.48 (.016)	.47 (.014)	.46 (.013)	.42 (.014)
Scalar product	.03 (.015)	.07 (.015)	.12 (.016)	.08 (.014)	.09 (.013)	.13 (.014)
Adjusted R^2	.387	.379	.354	.291	.296	.282
−Distance or −distance^2	.63 (.025)	.57 (.026)	.30 (.050)	.39 (.025)	.57 (.031)	.64 (.074)
Scalar product	.00 (.022)	.05 (.022)	.28 (.043)	.15 (.023)	.00 (.028)	−.06 (.065)
Penalty	.02 (.012)	.01 (.013)	−.09 (.024)	−.05 (.013)	.07 (.018)	.12 (.041)
Adjusted R^2	.387	.379	.355	.292	.297	.282
Mixed (raw scores)						
−Distance or −distance^2	.37 (.013)	.32 (.012)	.27 (.013)	.22 (.011)	.23 (.010)	.22 (.011)
Scalar product	.21 (.013)	.26 (.012)	.29 (.013)	.30 (.011)	.30 (.010)	.30 (.011)
Adjusted R^2	.306	.299	.286	.226	.229	.226
−Distance or −distance^2	.24 (.018)	.17 (.017)	.01 (.020)	.11 (.012)	.11 (.012)	.07 (.013)
Scalar product	.31 (.016)	.37 (.015)	.49 (.017)	.38 (.011)	.38 (.011)	.41 (.012)
Penalty	−.10 (.011)	−.13 (.011)	−.20 (.013)	−.17 (.009)	−.16 (.009)	−.18 (.010)
Adjusted R^2	.312	.308	.302	.248	.248	.245

Note: Entries for independent variables are standardized regression coefficients with standard errors in parentheses. The number of cases is 13,496 in analyses using individual party placements and 14,616 in those using mean party placements. The number of respondents is 1,928 and 2,088, respectively. The analyses include only respondents having valid scores for all four variables across all seven parties.

such differences separate my analysis from theirs: (1) its focus on choice and, consequently, on intrapersonal rather than interpersonal comparisons, (2) its use of individual rather than mean party placements, and (3) its consistent use of city-block rather than Euclidean or squared Euclidean distance in tests of the proximity model.

The analyses presented in Table 3 allow us to specify the effect of these differences. The table shows the results of testing the proximity model against the reduced and complete version of the directional model, respectively, under each of the 12 (2 × 2 × 3) method combinations derivable by means of the three factors. All 24 tests are multivariate regressions, with party evaluation as the dependent variable.

The first distinction—that between analyses based on an intrapersonal function and those based on raw scores—turns out to be the most important. The proximity model stands up well to extremely well against the directional model in the upper half of the table but loses most contests in the lower half. Note also that every regression in the upper half shows a better fit (higher R^2) than the corresponding regression in the lower half. This result is upheld in 11 out of 12 cases, even when the regressions are compared in terms of the absolute amount of variance explained rather than in terms of R^2.[27]

The two remaining distinctions are of some importance as well, although their effect does not rival that of the first. As expected, the use of mean party placements reduces the proportion of variance explained and tends to favor the directional model at the expense of the proximity model in most instances. Regressions taking squared Euclidean distance as a predictor—as in Rabinowitz, Macdonald, and Listhaug's multivariate confrontations of the two theories—consistently yield a worse fit than those specifying a linear distance effect. As a rule, they also put the proximity model at a disadvantage compared to the directional model (cf. Merrill 1995, 283). The contest between the two linear distance functions is more even, with the city-block specification showing a slightly better fit in tests based on individual party placements, and the Euclidean specification having a small edge when mean party placements are used.

In earlier sections, I showed that the intrapersonal comparisons required by choice theory *may* lead to a different outcome than do the interpersonal comparisons performed by Rabinowitz, Macdonald, and Listhaug. The above analysis demonstrates that they actually do so. Only one question remains: Why, specifically, does the one type of comparison not produce the same results as the other?

A first clue to the answer is given by Figure 4, which shows the relationship between party evaluation and party location on a left–right scale for each of ten different voter locations.[28] According to the proximity theory, the curves should peak at the point representing the voter's stand. The data show that they actually do so in nine out of ten cases, the tenth case falling but a hair's breadth off target. This result is far from self-evident. Rabinowitz, Macdonald, and Listhaug have repeatedly reported their failure to unravel curves of the kind displayed in Figure 4.[29] On the contrary,

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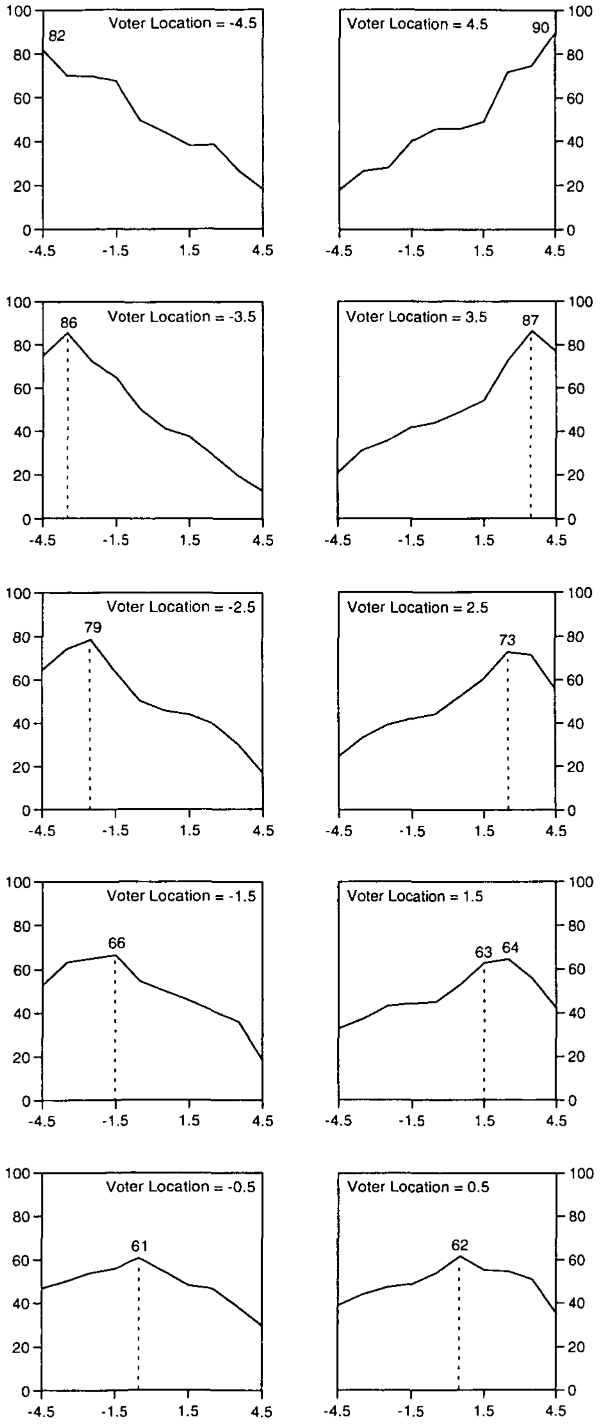
[27] Multiplying the values of R^2 in the upper half of Table 3 by .86 makes them comparable to those in the lower half in terms of the absolute amount of variance explained.

[28] For another analysis of the two theories based on the idea of keeping voter location constant while letting party location vary, see Granberg and Gilljam n.d.

[29] Rabinowitz and Macdonald 1989, 98–9, Figure 2, and 105–7, Figure 4; Rabinowitz, Macdonald, and Listhaug 1993, 5–6, Figure 2, and 11–2, Figure 3 to Figure 7; Listhaug, Macdonald, and Rabinowitz 1994a, 114–6, Figure 2, and 123–41, Figure 4 to Figure 9;

FIGURE 4. Mean Party Evaluation as a Function of Party Location on a Left–Right Scale, by Voter Location



they have always found evaluation to be a monotonic and more or less linear function of location, so as to conform to the pattern predicted by the scalar product.

The key to the paradox is largely to be sought in the choice of perspective. The diagrams in Figure 4 keep voter location constant while letting party location vary, thereby mirroring the idea of a voter choosing between rivaling parties. Those presented by Rabinowitz, Macdonald, and Listhaug, by contrast, keep party location constant while letting voter location vary, which is less informative from a choice point of view. Figure 4 also reveals why this shift of perspective makes a difference. Although voters generally prefer a party sharing their own stand, the absolute level of their enthusiasm varies considerably by voter location. For voters occupying one of the two most extreme slots on either side of the midpoint, the peak value is in the range of 82–90. For voters taking one of the two innermost stands on either side, the peak reaches no higher than 61–66. A closer look reveals that the pattern is not restricted to the peak values. In fact, voters far from the center tend to provide generally more positive evaluations *once proximity is held constant.*

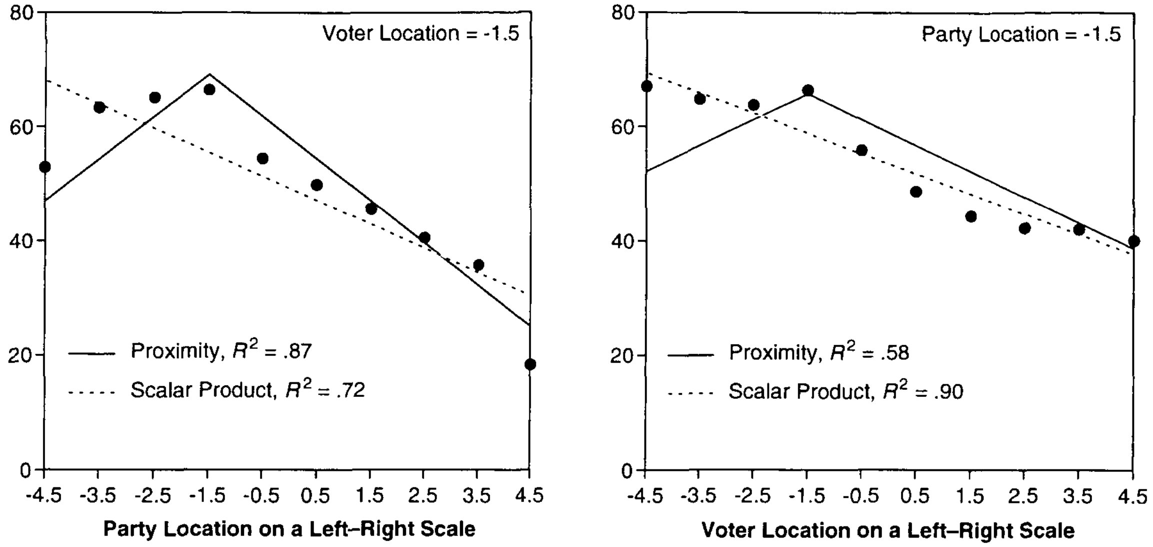
The likely cause of this phenomenon, which we may call the *eccentricity effect* will be discussed shortly. However, let us first, by means of Figure 5, consider its implications for the results derived on the basis of the two perspectives. The leftmost diagram keeps voter location constant (at −1.5) while letting party location vary, just as in Figure 4. The rightmost diagram keeps party location constant (at −1.5) while letting voter location vary, as in the analyses by Rabinowitz, Macdonald, and Listhaug. Note how the eccentricity effect conceals the proximity pattern in the latter case, making evaluation increase almost monotonically from right to left rather than climb to a peak and then decline.[30] As expected, a linear regression based on proximity fits the points of the leftmost diagram better than one based on the scalar product, whereas the opposite holds for the diagram to the right.

We may thus conclude that the eccentricity effect constitutes a plausible reason for the difference between my results and those of Rabinowitz, Macdonald, and Listhaug. Why, however, does this effect occur? The most credible explanation rests with the nature of the evaluation scores. In all likelihood, most respondents understand the thermometer items in agreement with the main design objective, that is, as a way of expressing their appreciation of each party *relative to the appreciation of other parties.* Consequently, each respondent adjusts the ratings so that a party located at a middling distance (compared to other parties) obtains a score near the neutral point of the scale (50). Parties closer than this benchmark are given higher ratings and those more distant lower. What is easily overlooked, however, is that the distance to the benchmark is not the same across respondents. Those far from the center will be farther away from their respective benchmarks than those in the middle ground.

Macdonald, Rabinowitz, and Listhaug 1995a, 3–4, Figure 2, and 10–3, Figure 3 to Figure 7.

[30] In many cases, including the one illustrated here, the pattern is somewhat sigmoid (S-shaped) or inverted sigmoid. See Granberg and Gilljam (n.d.), who, on the basis of Swedish data, report curves resembling a logistic function. See also the graphs presented by Merrill (1995).

FIGURE 5. Mean Party Evaluation as a Function of Party Location for a Given Voter Location and as a Function of Voter Location for a Given Party Location



When proximity is held constant, the ratings produced by eccentric respondents will therefore be higher than those of centric ones.

An illustration is provided by the two voters presented in Figure 2A. Assume that the *y* axis represents party evaluation and the *x* axis proximity. In keeping with the above rating scheme, the means of the evaluations supplied by the two voters are the same. However, voter 1 is located farther from the center than voter 2, which implies a lower mean as well as a higher variance in voter-party proximity. Due to the difference in mean proximity, the ratings provided by the first voter fall to the left of, and above, those provided by the second. Note also that the greater variation in the proximity scores of voter 1 yields a higher top rating and a lower bottom rating than for voter 2.

It remains but to subject my argument to a systematic test based on the full set of issue dimensions. If my reasoning up to this point is valid, then one simple modification of the analytical procedure normally used by Rabinowitz, Macdonald, and Listhaug should do much to redress the balance between the two theories. That such is actually the case is shown by the results presented in Table 4, which contains estimates for two series of statistical models. The first series emulates the type of pooled regression analysis commonly employed by Rabinowitz, Macdonald, and Listhaug. Consequently, the analysis is based on the original scores, without any subtraction of individual averages. Furthermore, it uses mean rather than individual party placements and a measure of proximity based on Euclidean distance. The second series is identical but for one modification: Proximity is measured in terms of its deviation from the individual mean.

As expected, the first series of estimates consistently favors the directional model. In the second series, however, the proximity model fares much better since most of the problems caused by the relative nature of the evaluation scores are now appropriately corrected. In fact, the simple adjustment of the proximity measure alone is in this case sufficient to shift the weight of evidence from one model to the other.

CONCLUSION

Few would deny that the Downsian theory has its shortcomings. As many have observed before me, however, a model is not likely to be replaced by

TABLE 4. Party Evaluation as a Function of Proximity, Scalar Product, and Penalty

Independent Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Proximity (raw score)	.42 (.007)	—	.23 (.010)	.41 (.008)	—	.11 (.011)
Scalar product	—	.44 (.007)	.30 (.010)	—	.46 (.007)	.38 (.010)
Penalty	—	—	—	−.03 (.008)	−.21 (.007)	−.17 (.009)
Adjusted R^2	.176	.197	.227	.177	.241	.246
Proximity (dev. from ind. mean)	.50 (.007)	—	.41 (.012)	.53 (.008)	—	.41 (.025)
Scalar product	—	.44 (.007)	.11 (.012)	—	.46 (.007)	.11 (.022)
Penalty	—	—	—	.06 (.008)	−.21 (.007)	.00 (.015)
Adjusted R^2	.250	.197	.254	.253	.241	.254

Note: Entries for independent variables are standardized regression coefficients with standard errors in parentheses. The number of cases is 14,817 and the number of respondents 2,135. The independent variables are constructed on the basis of mean party placements. Proximity is measured in terms of Euclidean distance.

anything but another model. According to Rabinowitz, Macdonald, and Listhaug, the directional theory is the solution for which we have been waiting. My analysis indicates that such is not the case. A worthy successor to the classical spatial model of electoral choice has yet to be found.

This main conclusion is based on four central observations: one theoretical, one methodological, and two empirical. The theoretical observation is that substantive theories cannot be evaluated merely by the extent to which they are compatible with the evidence. Their degree of falsifiability, that is, the extent to which they can, logically speaking, be refuted by empirical observations must also be taken into account. While this idea is hardly new, and while I cannot lay any claims to its origin however much I would want to, experience shows that it bears repeating. The largely unspecified nature of the region of acceptability and the penalty renders the directional theory less falsifiable than the proximity theory. The implication is that even if the directional model were found to be more compatible with empirical evidence, this would not be sufficient to demonstrate its general superiority.

The methodological observation is that we must learn to take choice theories seriously. A crucial feature distinguishing these from other types of theories is that the predictions they make concern the way an individual relates to a set of alternatives rather than the way an alternative relates to a set of individuals. The predictions should be tested accordingly, that is, by means of comparisons within rather than across individuals. Most of the tests performed by Rabinowitz, Macdonald, and Listhaug do not meet this requirement and therefore can neither refute nor corroborate either theory. The one exception to that rule does not qualify as serious evidence for other reasons (see Appendix C).

While the methodological point is worth making in its own right, irrespective of its consequences for the outcome, my empirical analyses demonstrate that it does matter. When the two theories are retested based on more appropriate methods, the results are different although the data are the same. This time, the evidence clearly supports the proximity model over the reduced and complete version of the directional model alike. By implication, there are reasons to believe that voters do not generally see issues as a dichotomous choice between a black and a white policy alternative, as assumed by the directional model, but are able to distinguish as well as express at least some shades of gray.

The final empirical section specifies the major reason why the intrapersonal perspective required by choice theory yields a different outcome than does the interpersonal perspective used by Rabinowitz, Macdonald, and Listhaug. A closer look at the data reveals an eccentricity effect, the consequence of which is a bias against the proximity model in tests based on interpersonal comparisons. Once this effect is taken into account, the proximity model fares much better even in analyses otherwise similar to those performed by Rabinowitz, Macdonald, and Listhaug.

Whereas Rabinowitz, Macdonald, and Listhaug have looked at evidence on the individual as well as the system level in their comparisons of the two theories, my own analyses have been confined to the individual level. My reasons for staying on that level are simple. Both theories are predicated on the idea that voter choice determines party strategy. Therefore, the extent to which the two theories are consonant with system-level evidence is of interest only insofar as they succeed on the individual level. Even if the directional model were found to be more compliant with system-level properties than the proximity model—a matter which, in my view, is still open for debate—it would lack interest as an explanation on that level unless it can assert itself as a model of voter choice.

Its failure to do so raises the question of how the shortcomings of the proximity model should instead be addressed. I shall resist the temptation to outline some possible answers to that question because my undertaking is not well suited to that end. As Popper (1968) points out, both conjectures and refutations are needed in the pursuit of scientific progress. Rabinowitz, Macdonald, and Listhaug contributed a conjecture. I have contributed a refutation. If Popper is right, the two efforts combined have brought us one small step nearer to the truth.

APPENDIX A: INCONSISTENCY OF THE PENALTY FUNCTION

Rabinowitz, Macdonald, and Listhaug (1991, 152) suggest that the penalty a voter applies to a party located outside the region of acceptability may vary with the voter's own stand. Voters who are more directionally sympathetic to a party, that is, those whose scalar product with the party is greater, might apply a weaker penalty than those who are less directionally sympathetic. They also present a class of utility functions that, in their view, satisfy this idea (Rabinowitz, Macdonald, and Listhaug 1991, 182, n5). For a party located outside the region of acceptability, this class of functions is defined as

$$U_{ij} = (r/|\mathbf{j}|)\mathbf{i}\cdot\mathbf{j} - k(|\mathbf{j}| - r)/(1 + e^{\mathbf{i}\cdot\mathbf{j}}),$$

where U_{ij} is the utility that voter i associates with party j, $\mathbf{i}\cdot\mathbf{j}$ the scalar product between the vectors extending from the origin to the points representing the issue stands of the voter and the party, $|\mathbf{j}|$ the length of the party vector, r the radius of the region of acceptability, and k an arbitrary positive constant. The penalty component contained in this function can be extracted by means of the general utility function of the directional model, that is, $U_{ij} = \mathbf{i}\cdot\mathbf{j} - P_{ij}$, where P_{ij} is the penalty applied by voter i to party j (Macdonald, Listhaug, and Rabinowitz 1991, 1110). By substitution we obtain

$$\mathbf{i}\cdot\mathbf{j} - P_{ij} = (r/|\mathbf{j}|)\mathbf{i}\cdot\mathbf{j} - k(|\mathbf{j}| - r)/(1 + e^{\mathbf{i}\cdot\mathbf{j}}),$$

which after rearrangement yields the penalty function:

$$P_{ij} = (1 - r/|\mathbf{j}|)\mathbf{i}\cdot\mathbf{j} + k(|\mathbf{j}| - r)/(1 + e^{\mathbf{i}\cdot\mathbf{j}}).$$

According to Rabinowitz, Macdonald, and Listhaug (1991, 151–2, 182, n5), the penalty function should meet three conditions for any voter and any party outside the region:

1. $\mathbf{i}\cdot\mathbf{j} - P_{ij} < (r/|\mathbf{j}|)\mathbf{i}\cdot\mathbf{j}$ (which represents the condition that the utility associated with a party located outside the region of acceptability must always be lower than that

associated with a party located on the boundary of the region in the same direction).

2. $P_{ij} \geq 0$ (which represents the condition that the penalty must be a penalty, not a reward).
3. $\partial P_{ij}/\partial(\mathbf{i} \cdot \mathbf{j}) \leq 0$ for all i given any particular j (which represents the condition that the penalty a voter applies to a certain party decreases as the voter becomes increasingly directionally sympathetic to the party).

The penalty function does satisfy the first condition, but it can easily be shown that it does not satisfy the latter two. To that end, it is assumed in the following that the location of party j remains constant at any given point outside the region of acceptability (in agreement with what is stipulated in condition 3) and that only voter location varies.

Consider first condition 2. The factor $(1 - r/|\mathbf{j}|)$ in the first term of the penalty function is always positive since $r < |\mathbf{j}|$ for any party outside the region of acceptability. Hence, $(1 - r/|\mathbf{j}|)\mathbf{i} \cdot \mathbf{j}$ takes on increasingly large negative values with increasingly large negative values of $\mathbf{i} \cdot \mathbf{j}$. The second term, by contrast, approaches the positive constant $k(|\mathbf{j}| - r)$ under the same conditions. Thus, at some point P_{ij} always falls below 0.

Condition 3 stipulates that the derivative of P_{ij} with respect to $\mathbf{i} \cdot \mathbf{j}$ must not be positive for any $\mathbf{i} \cdot \mathbf{j}$. The derivative can be written as:

$$\partial P_{ij}/\partial(\mathbf{i} \cdot \mathbf{j}) = (1 - r/|\mathbf{j}|) - [k(|\mathbf{j}| - r)/(1 + e^{\mathbf{i} \cdot \mathbf{j}})][e^{\mathbf{i} \cdot \mathbf{j}}/(1 + e^{\mathbf{i} \cdot \mathbf{j}})].$$

The first term is a positive constant. The first bracketed factor in the second term approaches 0 as $\mathbf{i} \cdot \mathbf{j}$ grows increasingly large, whereas the second bracketed factor approaches unity under the same condition. Hence, the entire second term approaches 0 as $\mathbf{i} \cdot \mathbf{j}$ grows increasingly large. The entire expression will, therefore, at some point always become positive.

That the function does not satisfy conditions 2 and 3 can also be shown through a simple example. Let us for convenience assume that the space is unidimensional. Assume in addition that the score of party j is 5, with $r = 4$, and $k = 1$. The penalty applied by a voter with a score of -5 would be

$$P_{ij} = (4/5 - 1)25 + 1(5 - 4)/(1 + e^{-25}) \approx -4,$$

and by a voter with a score of 5:

$$P_{ij} = (1 - 4/5)25 + 1(5 - 4)/(1 + e^{25}) \approx 5.$$

In other words, the penalty increases rather than decreases as the voter becomes more directionally sympathetic, and the first of the two examples does in fact result in a reward, not a penalty.

In all likelihood, it is impossible to devise a function that simultaneously satisfies all three conditions except by introducing thresholds, discontinuities, or boundary conditions that seem hard to justify on theoretical grounds. The root of the problem is that conditions 1 and 3 are in direct contradiction with each other. To satisfy condition 1, the penalty should increase with increasing positive values of $\mathbf{i} \cdot \mathbf{j}$, but to satisfy condition 3 it should decrease under the same circumstance.

APPENDIX B: REDUNDANCY OF THE MIXED MODEL

In order to perform a multivariate test of the two theories, Rabinowitz, Macdonald, and Listhaug suggest that we use a special statistical model, which they call a *mixed model*. The mixed model takes its point of departure in the formula for the squared Euclidean distance between a voter and a party, that is, $|\mathbf{i}|^2 + |\mathbf{j}|^2 - 2\mathbf{i} \cdot \mathbf{j}$, where the first two terms represent the squared lengths of the voter and party vectors and the third twice the scalar product. The squared Euclidean distance can thus be separated into two components: a length component, $|\mathbf{i}|^2 + |\mathbf{j}|^2$, and a scalar-product component, $-2\mathbf{i} \cdot \mathbf{j}$. The mixed model takes these components as its independent variables, albeit with their signs reversed so that the theoretically expected value of the corresponding regression coefficients becomes positive rather than negative. The model can thus be written as:

$$Y_{ij} = b_1 - b_2(|\mathbf{i}|^2 + |\mathbf{j}|^2) + b_3 2\mathbf{i} \cdot \mathbf{j} + u_{ij}.$$

According to Rabinowitz, Macdonald, and Listhaug, the empirical strength of the two theories can be evaluated by comparing and combining the coefficients of the model in various ways. If $b_2 = b_3 > 0$, we have a pure proximity model. If $b_2 = 0$ and $b_3 > 0$, we have a pure directional model. If $b_2 > 0$ and $b_3 > 0$ and $b_3 > b_2$, both theories enjoy at least a modicum of support. In order to assess their relative strength, Rabinowitz, Macdonald, and Listhaug suggest that we form the *model ratio* b_3/b_2. Although this ratio can in principle take on values less than 1, the interesting range is from 1, in the case of a pure proximity model, to infinity, in the case of a pure directional model.

When the mixed model was first introduced (Rabinowitz and Macdonald 1989), no rationale was provided for preferring it to a more conventional approach, that is, one taking (squared or unsquared) distance and scalar product as the predictors. In later works (Rabinowitz, Macdonald, and Listhaug 1993; Macdonald, Rabinowitz, and Listhaug 1995a, b), it is argued that the mixed model avoids the colinearity problems resulting from the fact that the scalar product is mathematically a component of Euclidean distance.

As I will demonstrate below, however, there are no reasons for preferring the mixed model to the conventional solution. On the contrary, the latter is superior on several counts. In order to keep my demonstration as short and simple as possible, I will ignore the special problems associated with the penalty factor (which is always left aside when Rabinowitz, Macdonald, and Listhaug use the model) as well as those related to the distinction between intrapersonal and interpersonal comparisons. Given this starting point, the alternative to the mixed model, which I will call the *standard model*, can be written as

$$Y_{ij} = b_4 - b_5 d_{ij} + b_6 \mathbf{i} \cdot \mathbf{j} + u_{ij},$$

where d_{ij} is the measure of (squared or unsquared) distance and $\mathbf{i} \cdot \mathbf{j}$ the scalar product. Compared to this alternative, the mixed model has three disadvantages.

First, in contrast to the standard model, the mixed model requires that we estimate the dependent variable as a *quadratic* function of *Euclidean* distance. Other (and better fitting) functional forms (e.g., linear) or distance conceptions (e.g., city block) cannot be accommodated or meet with difficulties. If, for example, we would like to estimate the dependent variable as a linear rather than quadratic function of Euclidean distance, the mixed model returns an equation which is nonlinear in the parameters. While such equations can still be estimated by resorting to special techniques, one is well advised to avoid them unless there are solid reasons to the contrary. The mixed model hardly constitutes such a reason.

Second, Rabinowitz, Macdonald, and Listhaug fail to specify a criterion whereby the mixed model and its associated model ratio lets us decide which of the two theories enjoys the strongest support. The standard model, by contrast, provides an unambiguous breakpoint. If the standardized version of b_5 equals that of b_6, support for the two theories is evenly balanced. If not, the theory corresponding to the higher coefficient has the upper hand.[31]

Third, in the standard model, the coefficients b_5 and b_6 represent the two theories in a direct and uncomplicated fashion. In the mixed model, by contrast, b_3 always has to be compared to b_2 before we can interpret its theoretical significance. This indirect relationship between the theories and the coefficients in the mixed model makes for needless difficulties of interpretation.

Let us finally consider the one argument put forth in support of the mixed model over the standard model, namely, that it is preferable as an estimation strategy because of the reduction of colinearity. If we let d_{ij} represent squared Euclidean distance, the standard model can be rendered in a form similar to that of the mixed model:

$$Y_{ij} = b_4 - b_5(|\mathbf{i}|^2 + |\mathbf{j}|^2) + (b_5 + 0.5b_6)2\mathbf{i} \cdot \mathbf{j} + u_{ij}.$$

Comparing this version of the standard model with the mixed model, we can immediately conclude that $b_1 = b_4$, $b_2 = b_5$, and $b_3 = b_5 + 0.5b_6$. It follows that all six coefficients can be estimated on the basis of either model. Irrespective of which model we use for estimation purposes, the estimates will be exactly the same, as will the associated standard errors. Furthermore, if we compare the standard errors of the coefficients of the two models, we find that they differ only in one case: The standard error of b_3 will be lower than that of b_6. This improvement is bought at the expense of the interpretability of b_3, however, and therefore yields no true benefit. Once we perform any of the operations required to interpret b_3, such as calculating $2(b_3 - b_2)$ to isolate the effect of the scalar product, the standard error of the resultant value becomes exactly the same as for the corresponding value derived on the basis of the standard model (in this case, the value of b_6).

APPENDIX C: PRIOR EVIDENCE REFUTED

The individual-level tests of the two theories reported by Rabinowitz, Macdonald, and Listhaug (1991) differ in several important respects from those the three authors have presented elsewhere. First, these tests are based on the proper form of comparison: intrapersonal rather than interpersonal. This is why they merit special consideration here. Second, the dependent variable is the actual vote rather than party evaluations. Third, and largely as a consequence of the first two differences, the region of acceptability and the penalty factor play a much more crucial role than they do in all other tests performed by Rabinowitz, Macdonald, and Listhaug. No special motivation for these departures from their normal testing strategy is provided.

The data come from the 1985 Norwegian and 1979 Swedish election studies.[32] The independent variables are based on placement on a left–right scale only. The region of acceptability is drawn so as to exclude the Socialist Left Party and the Progressive Party in Norway and the Left Party Communists in Sweden. The exact location of the border of the region is left unspecified. The penalty is operationalized so as to imply that no voter prefers a party outside the region. None of these choices are discussed or motivated, aside from the observation that the parties excluded are wing parties; see Figure C-1.[33]

In Rabinowitz, Macdonald, and Listhaug's view, the directional model proves superior in both countries. According to their analyses, it correctly predicts the vote in 64% of the Norwegian and 62% of the Swedish cases, versus 31% and 48%, respectively, for the proximity model (Rabinowitz, Macdonald, and Listhaug 1991, 171, Table 1, 177, Table 2). The following additional observations can be made.

1. The outcome is entirely dependent on how the region of acceptability and the penalty factor are operationalized. If, for example, all parties are considered to be inside the region, the proportion of votes correctly predicted by the directional model drops from 64% to 9% for Norway and from 62% to 20% for Sweden.
2. The operationalization decisions made ensure that all predictions of the directional model will benefit the two largest parties in either system: the Labor Party and the Conservatives in Norway, the Social Democrats and the Conservatives in Sweden. Of course, placing all bets on the known winners substantially increases the chance of obtaining a large proportion of correct predictions on the individual level. (On the party-system level, of which no account is taken in this part of Rabinowitz, Macdonald, and Listhaug's analysis, one simultaneously obtains the prediction that both Norway and Sweden have two-party systems, which obviously does not square very well with the evidence.)
3. Although the directional model provides a lot of leeway for the operationalization of the penalty factor, the one chosen here is inconsistent with its requirements. As repeatedly stressed by Rabinowitz, Macdonald, and Listhaug, the theory does not imply that a party outside the region will necessarily lose a contest, let alone every single vote, to *any* competitor inside the region.[34] The operationalization used in this case, however, forces such an outcome.
4. While the theory also provides a lot of latitude for the operationalization of the region of acceptability, the one

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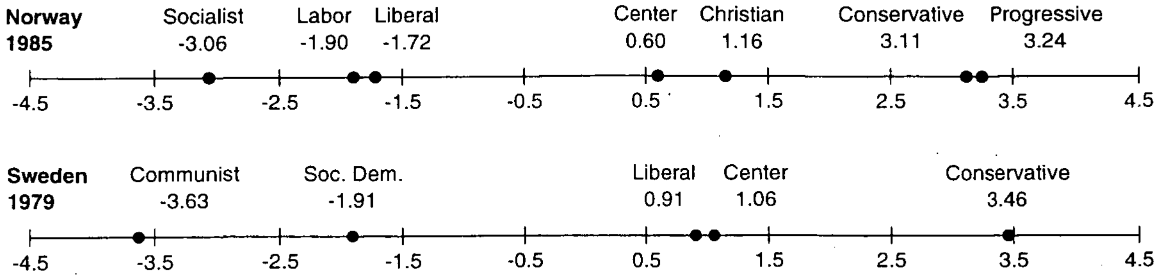
[31] Standardizing the coefficients of the mixed model, as in Rabinowitz and Macdonald 1989, is of no help here since the model's rules of interpretability, e.g., the equality between b_2 and b_3 in the case of a pure proximity model, hold for the unstandardized coefficients only. Standardizing b_5 and b_6 in the standard model is a meaningful operation, however, since these two coefficients represent the two theories and their associated measures in a direct fashion. A solution to the lack of a parity criterion for the mixed model is proposed by Merrill and Grofman (1997) in the course of developing a unified model for testing spatial choice theories. Regrettably, their unified model, like the mixed model, operates on the basis of squared Euclidean distance only.

[32] The two studies—conducted by Henry Valen and Bernt Aardal in Norway and by Sören Holmberg in Sweden—are both based on nationally representative samples of all members of the electorate aged 18–80. Data were successfully collected, predominantly by means of personal interviews, from 2,180 respondents in Norway and 2,816 in Sweden, which corresponds to response rates of 72.7% and 80.5%, respectively. The data sets are available from the Norwegian Social Science Data Services (NSD) and the Swedish Social Science Data Service (SSD).

[33] The Norwegian scale is originally scored from 1 to 10 and the Swedish from 0 to 10. Both were rescored to range from −4.5 to 4.5 in all analyses presented here.

[34] According to Rabinowitz, Macdonald, and Listhaug (1991, 152), a party outside the region is sure to lose only against a competitor located on the boundary in the same direction. Since they do not specify the precise location of the boundary, it is unclear whether such a party is assumed to exist in the present case. What is clear, however, is that boundary parties cannot exist on both sides simultaneously, since the theory requires that the boundary fall at a *directionally invariant* distance from the origin. This suffices to make the operationalization untenable.

FIGURE C-1. Mean Placement of Parties on a Left-Right Scale: Norway 1985 and Sweden 1979



used here is nevertheless inconsistent with it. The theory imposes but a single requirement, namely, that the region be located at a *directionally invariant* distance from the neutral point. Still, in the Norwegian case, the Socialist Left Party is placed outside the region but the Conservatives inside it, despite the former being closer to the neutral point than the latter according to Rabinowitz, Macdonald, and Listhaug's own choice of measurement (see Figure C-1).

5. The Swedish Conservatives are in fact more distant from the neutral point than the Norwegian Socialist Left and Progressive parties (see Figure C-1). Still, the former is placed inside the region, while the latter two are placed outside. Cross-national differences in the size of the region are not ruled out by the theory, but one would have expected Rabinowitz, Macdonald, and Listhaug to motivate explicitly and convincingly the presence of such a difference in the case at issue in order to comply with the demand of fair testing.
6. Drawing the boundary of the region of acceptability so as to separate parties that are as similar in terms of distance from the neutral point as are the Socialist Left, the Progressive, and the Conservative parties in Norway or the Left Party Communists and the Conservatives in Sweden must be considered a questionable testing strategy, at least when the consequences of placing a party inside or outside the region are as drastic as in the present case.
7. Although the directional theory predicts that respondents placing themselves at the neutral point should be indifferent to all parties, Rabinowitz, Macdonald, and Listhaug contrive a prediction that such respondents should, in the Swedish case, all prefer the Social Democrats. (In Norway, the scale used did not allow respondents to place themselves at the neutral point.) The prediction is based on the presumption that all the neutral respondents are in fact slightly left-wing, since there are more respondents in the category just to the left of the neutral point than in the one just to the right. Rabinowitz, Macdonald, and Listhaug thus ensure that 42% of the predictions for the neutral respondents will be correct as opposed to 9% if these respondents had instead been counted as slightly right-wing. As pointed out by Granberg and Gilljam (n.d., n4), the same reasoning would have led to but 9%, 7%, and 5% correct predictions, respectively, if applied to the 1985, 1988, or 1991 Swedish election study. In those years, the respondents just to the right of the neutral point outnumbered those just to the left, whereas the proportion of neutral respondents voting for the Social Democrats and Conservatives was approximately the same as in 1979.
8. Rabinowitz, Macdonald, and Listhaug's reliance on mean rather than individual party placements in constructing the main independent variables—proximity and scalar product—leads, in combination with other factors, to absurd consequences in the test of the proximity model. For example, it becomes theoretically impossible for a Norwegian voter to occupy such a position that Labor becomes the most proximate party. At −2.5, the Socialist Left Party is the closest, and at −1.5 the most proximate location is already taken by the Liberals (see Figure C-1).[35]

Due to the above weaknesses, I find it difficult to accept the results reported by Rabinowitz, Macdonald, and Listhaug (1991) as serious evidence in the contest between the directional and the proximity model. The methods described in the main text of this article (those underlying Table 2) allow a more reliable verdict. Among other things, these methods limit (although they cannot completely eliminate) the effect of more or less arbitrary decisions regarding the boundary of the region of acceptability by making use of the entire preference order rather than the vote alone. Furthermore, the region and the associated penalty are operationalized in agreement with theoretical requirements.[36] No ad hoc predictions of the type described under point seven above are added. Finally, artifacts of the type exemplified for Norwegian Labor voters are avoided.

The results are shown in Table C-1.[37] The substantive conclusions are the same as those reached in the main text. Irrespective of whether we look at Norway 1985 or Sweden 1979 and whether we include the penalty factor or not, the evidence clearly favors the proximity model. Again, this result also generalizes to the actual vote. The proximity model correctly predicts the vote in 54% of the Norwegian cases and 61% of the Swedish. The corresponding results for the directional model are 20% and 34% based on the scalar product alone. With the penalty factor taken into account, the figures are 49% and 56%.[38]

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[35] This problem passes unnoticed in the analysis of Rabinowitz, Macdonald, and Listhaug (1991). The prediction for a voter score of −2.5 is, without comment, accorded the Labor Party rather than the Socialist Left.

[36] In agreement with the practice followed by Rabinowitz, Macdonald, and Listhaug in their analysis of the Norwegian 1989 data set, the border of the region of acceptability is drawn so as to just enclose the central cluster of parties based on the mean party placements, that is, those located at a distance of between 0.60 and 1.91 from the neutral point according to Figure C-1. The border is thus drawn at 1.92.

[37] Party evaluations in Sweden were obtained on a dislike–like scale ranging from −5 to 5 rather than by means of the 0–100 feeling thermometer used in Norway.

[38] The methods underlying these percentages are analogous to those described in footnote 26.

TABLE C-1. Party Evaluation as an Intrapersonal Function of Proximity, Scalar Product, and Penalty: Norway 1985 and Sweden 1979

Country and Independent Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Norway 1985						
Proximity	.65 (.007)	—	.62 (.012)	.69 (.009)	—	.81 (.023)
Scalar product	—	.53 (.008)	.04 (.012)	—	.52 (.008)	−.12 (.019)
Penalty	—	—	—	.08 (.009)	−.26 (.008)	.14 (.013)
Adjusted R^2	.417	.283	.417	.421	.353	.423
Sweden 1979						
Proximity	.76 (.006)	—	.75 (.011)	.78 (.007)	—	.84 (.019)
Scalar product	—	.63 (.008)	.02 (.011)	—	.60 (.007)	−.05 (.016)
Penalty	—	—	—	.04 (.007)	−.33 (.007)	.07 (.011)
Adjusted R^2	.579	.394	.579	.580	.502	.581

Note: Entries for independent variables are standardized regression coefficients with standard errors in parentheses. The number of cases is 12,327 for Norway and 12,275 for Sweden, and the number of respondents 1,761 and 2,455, respectively. The analyses include only respondents having valid scores for all variables across all parties.

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Culture and the Environment in the Pacific Northwest

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Douglas and Wildavsky argue that environmental activism is rooted in an egalitarian cultural bias. Others, like Paehlke, counter that environmental commitments and concerns are autonomous from redistributive concerns. Students of the "New Politics" agree that environmentalism is autonomous from conventional left-right distributive concerns but also argue that environmental attitudes and beliefs are embedded in "postmaterial" values, such as citizen participation. Still other scholars emphasize a cultural consensus around environmental values and beliefs. What distinguishes environmental activists, in this view, is less what they believe than their willingness to make sacrifices for those values and beliefs. Drawing upon several surveys of environmental groups and the mass public in the Pacific Northwest, we test these four hypotheses and find that the Douglas-Wildavsky "cultural theory," although not without its limitations, appears to provide the more satisfactory account of environmental preferences.

Empirical studies of environmental attitudes often ask what people want (Dunlap, Gallup, and Gallup 1993; Dunlap and Scarce 1991) but less frequently ask why. Perhaps it is because the answer seems straightforward, even simple. People want cleaner air and water, wilderness and species preservation, habitat protection, and a healthier, safer earth because these are so obviously desirable. Environmentalism would seem to be one preference that needs no explanation.

Attempts to explain environmental preferences and concerns in terms of standard demographic variables have generally not fared well (Jones and Dunlap 1992, Van Liere and Dunlap 1980).[1] Perhaps this is because the almost universal support that environmentalism now commands leaves little variance to be explained. Or perhaps we have been looking in the wrong places, asking the wrong questions.

In this paper, building upon the so-called cultural theory of Douglas (1970, 1982a) and Wildavsky (1987; Thompson, Ellis, and Wildavsky 1990), we investigate the cultural sources of environmental attitudes and beliefs. What values and beliefs, if any, underlie environmental commitments? What sorts of people defending what sorts of social relationships tend to become environmentally active? When environmental activists urge us to conserve energy or recycle plastics, what else if anything are they trying to tell us about how we should live with one another?

The Douglas-Wildavsky theory of cultural bias posits four basic ways of organizing and justifying social and political life: egalitarian, individualistic, hierarchical, and fatalistic. Each of these cultural biases is "a point of view, with its own framing assumptions and readily available solutions for standardized problems" (Douglas 1997, 128). Adherents of each cultural bias construct their policy preferences so as to bolster their preferred pattern of social relationships. Perceptions about environmental risks and dangers, in this view, are embedded in cultural orientations, rather than being merely a function of the level of information about the safety of particular technologies, or a product of generalized psychological predispositions, such as risk acceptance or aversion (Dake and Wildavsky 1990, Douglas 1975, Douglas and Wildavsky 1982, Rayner 1992).

Environmental activists, according to this cultural theory, are worried about the greenhouse effect or deforestation, for example, not only because they are concerned about the fate of the earth but because they desire to transform how human beings live with one another in an egalitarian direction. To accept that nature is fragile and that the slightest misstep may result in cataclysmic consequences for the human species helps egalitarians to justify a politics that would dramatically curtail the activities of the restless entrepreneur in the name of the collectivity. Competitive individualists construct a view of nature as forgiving and resilient because it makes it easier for them to justify laissez-faire and to resist those who would enhance centralized, governmental control. Or, to take another example, the idea of resources as nonrenewable and rapidly depleting is hypothesized as appealing to egalitarians because it enables them to justify regulating and redistributing limited resources. In order to

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[1] Surveying National Opinion Research Center data on support for spending on the environment from 1973 to 1990, Jones and Dunlap (1992) found that age, education, income, gender, race, residence at age 16, current residence, occupational prestige, industrial sector, as well as political ideology and political party, taken all together, could not explain more than about 9% of the variance. Only age (Pearson's $r = -.23$) and to a much lesser extent education ($r = .14$) and ideology ($r = .14$) showed much of a relationship with support for environmental spending.

resist these encroachments on the process of bidding and bargaining, adherents of a competitive individualistic culture counter by focusing on the "resource-full" nature of individual entrepreneurs (Thompson, Ellis, and Wildavsky 1990, 26–7, 62). The debate about nature, in this view, is also fundamentally a contest over cultures.

This thesis has attracted widespread attention in the area of risk analysis (see Rayner 1992), understandably so since it challenges a number of settled beliefs about the perception of environmental dangers. It runs counter to the seductively simple idea that people worry about environmental harms because they are harmful. Environmental activism, in this view, has little or nothing to do with culture and everything to do with the sorry state of the environment. Alternatively, we are often told that people begin to worry about the environment when they reach a higher stage of consciousness, either because they have at last been educated about the real harms confronting the environment, or because they have satisfied their primary subsistence and economic needs. The Douglas-Wildavsky cultural thesis, by insisting that perceptions of environmental harm are "defined, perceived, and managed" (Rayner 1992, 84) according to a cultural bias, is unsettling to many received ideas about risk perception. For the most part, however, challenges to the theory and the contestation surrounding it have remained at a theoretical level.[2]

Among the ideas that the Douglas-Wildavsky hypothesis challenges head-on is the widespread belief that environmentalism transcends conventional political ideology. The view that environmentalism is "inherently neither left nor right" has been cogently argued in Paehlke's important book, *Environmentalism and the Future of Progressive Politics* (1989, 177). Paehlke suggests that we need to draw "a distinction between the 'distributive' politics of the traditional political spectrum and environmental politics" (p. 178). Distributive politics, as he defines it, "is concerned with distribution and redistribution of the products and other intended benefits of economic activity." The ethical debate in this sphere is "carried out in terms of equity." Environmentalism, according to Paehlke's autonomy thesis, offers a totally separate discourse; an ideology that is altogether apart from the conventional left-right continuum (p. 7). Environmental politics, Paehlke explains, "competes with the whole distributional agenda" (p. 189). To map the contemporary ideological spectrum properly, he argues that one needs two dimensions. The first is the familiar left-right dimension that turns on distributive economic concerns; the second is completely independent and runs from pro- to anti-environmentalism (p. 190, also p. 178; cf. Cotgrove 1982, 112, and Milbrath 1984, 24).

A related but different version of the autonomy thesis is argued by students of the "New Politics." Largely drawing upon data derived from Western Europe, they agree with Paehlke that environmentalism is "orthogonal" to the materialistic, equity-based politics of the Old Left, but unlike Paehlke these scholars embed environmentalism in the emergence of a new left-right framework pivoting around "postmaterial" issues, such as participation and individual freedom (Dalton 1994, esp. 121–34; Inglehart 1977, 1990). The environmental movement, like other "new social movements," such as the peace and the women's movement, is said to be driven by a new "elite-challenging" as opposed to "elite-directed" style of politics (Inglehart 1977, 3). In this view it is citizen participation and opposition to elite authority, more than equal distribution of resources, that is the motive power behind environmental commitment.[3]

A third and dramatically different alternative to the Douglas-Wildavsky cultural hypothesis is offered by Kempton, Boster, and Hartley in their recent study, *Environmental Values in American Culture* (1995). Whereas Douglas and Wildavsky argue that environmental activism is strongly joined to egalitarianism, Kempton and his colleagues counter that there is "a single cultural consensus" on environmental values and beliefs, and those few who do not share this consensus do not adopt a coherent and consistent alternative. By "cultural consensus" they do not mean that every last person agrees with each and every environmental value or belief; instead, they mean that "there is only one set of culturally agreed upon answers" (p. 211; also 196–7). The general public, in this view, closely resembles environmental activists. Even members of the radical environmental group, Earth First!, have "more or less the same beliefs and values as other Americans." What differentiates environmental activists from the general public, according to Kempton et al., is less what they believe and value than their willingness to make personal sacrifices for their values and beliefs (p. 209). Far from being part of an egalitarian challenge to dominant American values and institutions, environmental activism is the American cultural mainstream.

DATA AND METHODS

To test these rival hypotheses, we carried out several surveys. The first went to the northwestern U.S. membership of three environmental groups: Audubon Society, Sierra Club, and the Earth Island Institute. The first two are well-known mainstream environmental groups, while the third is an avowedly nonmainstream organization that defines itself as working at the intersection of social justice and ecology issues.[4] For comparative purposes, we sent the same survey to a small group of leaders in the Sierra Club and Earth Island

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[2] Recently, there has been closer empirical scrutiny. See, for example, Marris, Langford, and O'Riordan 1996; Grendstad and Selle 1997.

[3] Environmentalism was originally no more than a passing footnote to Inglehart's "postmaterialism" thesis (1971, 1012), but the environmental movement is now generally considered "the archetypical example of postmaterial politics" (Dalton 1994, xiii).

[4] Our original plan was to survey at least one mainstream and one nonmainstream group. We chose these three primarily because they were the ones that granted us access to their membership list. We also hoped to survey a so-called wise-use group, but we were not successful in gaining access to its membership list.

TABLE 1. Correlates of Environmentalism

	Environmental Activists		Salem-Yamhill Public	
	New Ecological Consciousness	Spending on Environment	Environmental Concern	Spending on Environment
Egalitarianism	.39** (.31**)	.36** (.19**)	.53** (.38**)	.47** (.33**)
Individualism	−.34**	−.36**	−.43**	−.37**
Hierarchy	−.09*	−.13**	−.16**	−.13*
Ideology	.29**	.36**	.39**	.38**
Party ID	.24**	.34**	.38**	.37**
Participation	.09*	−.02	NA	NA
Age	−.08*	−.10**	−.06	−.03
Education	−.02	.10**	.05	.09
Gender	−.06	.00	−.21**	−.08
Income	−.13**	−.04	−.04	.07

Note: Correlation coefficients are Pearson's r. In parentheses are partial correlations, controlling for ideology and party identification. Variables are coded so that positive values indicate liberal, Democrat, and male. $^{*}p < .05$; $^{**}p < .01$; two-tailed test.
NA = Not asked in the survey.

Institute. We also mailed a much shorter version to a random sample of residents in Oregon's capital, Salem, and the adjacent, largely rural, Yamhill County. On occasion, for comparative purposes only, we also draw upon a survey of members of the radical environmental group, Earth First! More specific information on sampling methods and the nature of each sample is given in Appendix A.

Respondents were offered a series of statements designed to measure egalitarian, individualistic, and hierarchical cultural biases as well as a range of environmental attitudes and beliefs.[5] For each item, respondents were asked to place themselves on a seven-point Likert-type scale, ranging from strongly agree to strongly disagree. *Egalitarianism*, *Individualism* (which we also call *Market Individualism*), and *Hierarchy* were each measured by three-item scales (see Appendix B for the items used in this and other scales, as well as for the reliabilities of all scales). To measure support for the environment, in both samples we used a single item: "We are spending too little money on improving and protecting the environment."

In the environmental group sample,[6] we also constructed a ten-item scale modeled on the New Environmental Paradigm pioneered by Dunlap and Van Liere (1978) and used by Milbrath (1984) and others. The central elements of this paradigm that we attempted to measure are anti-anthropocentrism and the beliefs that there are limits to growth, nature is fragile, the possibility of ecological catastrophe is imminent, and a fundamental transformation is needed in the way we live our lives (Dunlap et al. 1992, 4, 6; Milbrath 1984, 44–8). For the Salem-Yamhill general public, we constructed a five-item scale designed to measure level of environmental concern.

IS ENVIRONMENTALISM IDEOLOGICALLY AUTONOMOUS?

We begin by testing whether environmental attitudes and beliefs constitute a relatively autonomous sphere of ideas or are embedded in cultural biases and/or in the conventional left-right ideological spectrum. First, we examine the correlations between egalitarian or distributive concerns, on the one hand, and environmental commitments, on the other. Second, we look at the relationship between environmentalism and the participatory, "elite-challenging" New Politics. Finally, we investigate the relationship between market individualism and environmentalism.

Equality

If Paehlke is correct that environmentalism constitutes a relatively autonomous sphere of ideas, then we would expect to find a weak relationship between environmentalism and egalitarianism. If Douglas and Wildavsky's theory of cultural bias is correct, then the correlation should be strong. As Table 1 shows, among both environmental activists and the Salem-Yamhill general public there are consistently robust relationships between egalitarianism and environmentalism. Those concerned about inequality are substantially more likely to show concern about environmental degradation and to favor spending more on environmental protection.[7]

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[5] We made no effort to measure Wildavsky's fourth category, "fatalism," largely because previous survey research (Jenkins-Smith 1994a, 16) had found it to be inconsequential in understanding environmental attitudes.

[6] Throughout this article, we often refer to the Sierra Club, Earth Island, and Audubon members surveyed as "environmental activists." Although not all members of environmental groups are activists in the usual sense of that term, the great majority of our respondents deserve that label. During the past five years, more than 60% had attended an environmental group meeting; slightly more than half had done volunteer work for an environmental group; almost three-fourths had written a letter to a legislator regarding an environmental issue; and 80% participated in a product boycott because of the company's record on the environment. The only political activity reported by less than a majority was writing a letter to the editor about an environmental issue, done by fewer than one-third. Of these five political activities, at least two were reported by 83% of respondents and four or five were reported by 40%.

[7] The stronger relationship for the Salem-Yamhill group is likely due largely to the less restricted range of environmental responses among the public as compared to the activists. The standard deviation on the

Paehlke's autonomy thesis does not fare well, even if we use the conventional liberal-conservative self-designation (see Table 1). Among both activists and the public, there are generally strong correlations between environmental attitudes and ideological *Self-designation*, as well as between environmental attitudes and *Party Identification* (also see Guth et al. 1995, esp. 372; and Dunlap et al. 1992, 10). Liberals and Democrats are more likely to express ecological consciousness, show strong environmental concern, and support spending for environmental protection.

If ideological self-designation and party identification account for environmental attitudes so well, then why introduce the concept of egalitarianism? To test whether egalitarianism contributes to our understanding of environmentalism, we looked at the partial correlation between the two, controlling for liberal-conservative self-identification and party identification. In both populations, as Table 1 shows, egalitarianism explains significant additional variance in environmental attitudes even after controlling for ideological self-designation and party identification.

Participation and Authority

The New Politics argument rests on three distinct claims: (1) The New Politics issues of participation and authority are independent of the Old Politics issues of redistribution; (2) environmental attitudes and beliefs are autonomous from the old redistributive politics; and (3) environmental attitudes and beliefs are embedded in the New Politics issues of participation and authority. We have already seen that the second of these claims is not correct, but what about the other two? The theory still could be vindicated if the relationship between environmentalism and the New Left value of participation is greater than the relationship between environmentalism and the Old Left value of equality.

The environmental activists were offered a classic New Politics statement: "We should all participate in each decision that directly affects us."[8] Consistent with the New Politics argument, *Participation* is only weakly related to egalitarianism (Pearson's r = .14), and it is completely unrelated to ideological self-designation and party identification. But contrary to the New Politics argument, as Table 1 shows, the importance respondents attach to participation also shows no relationship with support for environmental spending and virtually none with the ten-item measure of ecological consciousness. The value activists place on participation, in short, has little or nothing to do with their environmental beliefs or attitudes.[9]

Another way to test the New Politics thesis that environmentalism is distinguished by its "elite-challenging" character is to examine the relationship between environmentalism and our hierarchy scale, which includes such statements as "one of the problems with people today is that they challenge authority too often" and "society works best when people strictly obey all rules and regulations." The elite-challenging hypothesis leads us to expect a strong inverse relationship between hierarchy and environmental support. Table 1 does show a consistently inverse relationship, but it is uniformly weak. Elite-challenging attitudes, then, do not appear to be strongly related to environmental attitudes, either among the general public or among activists.[10]

Market Individualism

What about attitudes toward the market? Is the new environmentalism at bottom the old antimarket impulse? (Or, looked at differently, is opposition to environmentalism grounded in traditional promarket attitudes?) Among both the Salem-Yamhill public and activists, Table 1 shows, there is a strong inverse relationship between support for market individualism and support for environmentalism. It is, in fact, almost the mirror image of the relationship between egalitarianism and environmentalism, which raises the question whether individualism and egalitarianism are not better conceived as opposite ends of a continuum rather than separate dimensions. Certainly the egalitarianism and individualism scales are inversely correlated (Pearson's $r = -.48$ in the activist sample and $-.44$ in the Salem-Yamhill public).[11] May it not be more appropriate, then, to combine the items into a single scale? One way of addressing this question is to enter egalitarianism and individualism in a regression equation simultaneously to determine whether any unique additional variance in environmental attitudes is explained by including a second measure of cultural bias. As Table 2 reports, both individualism and egalitarianism help explain significant unique variance in environmental attitudes. Moreover, in both samples, adding any one of the individualist items to the three-item egalitarian scale reduces the reliability of the scale. Finally, in both samples, factor analysis with varimax rotation revealed two distinct factors, one comprising all three egalitar-

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environmental spending item was 1.18 among the activists, compared to 1.87 for the public. Similarly, the standard deviation on the ten-item measure of ecological consciousness was 0.99, in contrast to 1.50 for the five-item measure of environmental concern used in the general public survey. The standard deviation for each item used in these scales is reported in Appendix B.

[8] One of Inglehart's (1977, 399–400) two measures of "postmaterialism" is: "Give the people more say in important governmental decisions." Because our participation item proved to be so weakly related to environmental attitudes and beliefs, we dropped the item in the Salem-Yamhill survey.

[9] Nor can the weakness of this relationship be attributed simply to the restriction of range on the participation item. The range and standard deviation (1.49) on the participation item are identical to those for another item: "We need to dramatically reduce inequalities between the rich and the poor, whites and people of color, and men and women" (see Table A-3). Yet, the Pearson's r correlation between this egalitarian measure and the environmental spending item is .33, and the correlation with the new ecological consciousness scale is .28.

[10] When one controls for other variables, as Table 3 shows, these weak relationships are even further attenuated or, as is the case among activists, reversed.

[11] These results are virtually identical to those found by Marris, Langford, and O'Riordan (1996, Table 6.2).

TABLE 2. Standardized Partial Regression Coefficients for Egalitarianism and Individualism

	Environmental Activists		Salem-Yamhill Public	
	New Ecological Consciousness	Spending on Environment	Environmental Concern	Spending on Environment
Egalitarianism	.28	.24	.42	.38
Individualism	−.21	−.23	−.25	−.21

Note: All relationships are significant at $p < .01$; two-tailed test.

ian items, the other composed of the three individualist items (see Appendix C). Thus, the two measures' factor structure, internal consistency, and nonredundant predictive power justify treating them as measures of distinct albeit related constructs.

The explanatory importance of market individualism and egalitarianism holds up even when we enter these two cultural bias variables into a regression equation together with the standard demographic variables (*Age*, *Income*, *Gender*, and *Education*) as well as party identification, ideological self-designation, and the hierarchical cultural bias. In both the environmental activist and general public sample, as Table 3 documents, egalitarianism and market individualism consistently emerge as the variables that explain the greatest amount of variance in environmental attitudes. This is further support for the Douglas-Wildavsky cultural thesis.

IS THERE A CULTURAL CONSENSUS AROUND ENVIRONMENTALISM?

Kempton, Boster, and Hartley's (1995) cultural consensus thesis leads us to expect relatively small differences between the public and activists in both environmental attitudes and cultural biases. Douglas and Wildavsky's cultural bias thesis, in contrast, leads us to expect large differences (Wildavsky 1991). Moreover, Douglas and Wildavsky's cultural theory also leads us to predict that the more egalitarian the environmental group, the more environmentally concerned the members of that group will be, whereas the consensus thesis leads us to expect environmental groups, even the more radical among them, to be quite similar in attitudes.

Previous research has consistently shown widespread support for environmental protection among the general public (Brick, Hannigan, and Krueger 1995; Dunlap and Scarce 1991). Our results are consistent with those findings. Generally, as Table 4 documents, the Salem-Yamhill respondents show high levels of environmental concern. Although the public clearly worries about environmental degradation and values environmental protection, it is not surprising that they are less intensely concerned than activists. Only about one in eight Salem-Yamhill respondents, for instance, agrees in the strongest possible terms that we will soon experience an ecological catastrophe, compared to about half of the Earth Island sample and one in three Audubon and Sierra Club respondents. These findings seem to support the consensus thesis that there is a general proenvironmental sentiment, and what sepa-

TABLE 3. Standardized Partial Regression Coefficients for Cultural Bias, Political Self-Identification, and Demographic Traits

	Environmental Activists		Salem-Yamhill Public	
	New Ecological Consciousness	Spending on Environment	Environmental Concern	Spending on Environment
Cultural bias				
Egalitarianism	.28**	.14**	.31**	.28**
Individualism	−.22**	−.19**	−.22**	−.22**
Hierarchy	.14**	.08	−.11*	−.05
Political self-identification				
Ideology	.08	.14*	.07	.10
Republican	−.05	−.14**	−.11	−.14
Democrat	−.13*	−.07	−.02	−.01
Demographic traits				
Age	−.01	−.02	−.02	.03
Education	−.04	.06	−.01	.02
Gender	−.09*	.01	−.04	.06
Income	−.01	.04	−.05	.02
Multiple *R*	.44	.42	.61	.56
Standard error	.88	.98	1.20	1.55
R^2, All variables	.20	.18	.37	.32
R^2, Egalitarianism and Individualism only	.18	.17	.33	.25
R^2, Egalitarianism only	.15	.13	.28	.22

*$p < .05$; **$p < .01$; two-tailed test.

TABLE 4. Percentage Endorsing Environmental and Cultural Statements

	Salem-Yamhill	Audubon Society	Sierra Club	Earth Island	Sierra Leaders	Earth Island Leaders	Earth First!
Environmental concern							
1. If things continue on their present course, we will soon experience a major ecological catastrophe.	64	81	86	91	100	93	NA
2. The oceans are gradually dying from oil pollution and dumping of waste.	67	75	87	92	84	86	95
3. We are fast using up the world's natural resources.	78	94	95	97	100	93	NA
4. We are spending too little money on improving and protecting the environment.	57	86	93	96	100	97	94
Technology and deep ecology							
1. Science and technology provide the human race with its best hope for the future.	61	56	55	34	45	21	17
2. Humans are no more important than any other species.	32	51	53	60	61	52	NA
3. We would be better off if we dramatically reduced the number of people on this earth.	40	63	68	76	88	83	NA
Cultural bias							
1. What our country needs is a fairness revolution to make the distribution of goods more equal.	32	38	41	59	47	70	71
2. The world would be a more peaceful place if its wealth were divided more equally among nations.	33	48	53	72	50	86	64
3. We need to dramatically reduce inequalities between the rich and the poor, whites and people of color, and men and women.	66	80	82	89	85	97	NA
4. Competitive markets are almost always the best way to supply people with the things they need.	76	59	47	28	34	34	17
5. Society would be better off if there was much less government regulation of business.	55	31	12	10	9	7	NA
6. People who are successful in business have a right to enjoy their wealth as they see fit.	76	38	34	24	27	17	13
7. Society works best when people strictly obey all rules and regulations.	65	50	31	26	30	7	15
8. Respect for authority is one of the most important things that children should learn.	87	72	46	39	42	24	NA
9. One of the problems with people today is that they challenge authority too often.	44	26	13	10	3	3	NA

NA = Not asked in the survey.

rates activists from the public is primarily the intensity with which they hold a belief rather than what they believe.

Other items, however, measuring attitudes toward science and technology and "deep ecology," reveal sharper differences, not only between the public and the activists but also among the activists themselves. Table 4 shows that Salem-Yamhill respondents have a strong faith in the promise of science and technology to solve human problems, a faith largely shared by Audubon and Sierra Club members. Earth Islanders and especially members of Earth First! take a far more jaundiced view of science and technology (cf. Milbrath 1984, 31). There is also nothing approaching a cultural

TABLE 5. Mean Score on Cultural Bias Scales

	Egalitarianism	Hierarchy	Individualism
Salem-Yamhill	−.13	.77	.92
Audubon			
Inert (*N* = 47)	.32	.49	.50
Active (*N* = 101)	.57	.19	−.24
Highly active (*N* = 61)	.46	−.17	−.45
Sierra Club			
Inert (*N* = 46)	.41	−.37	−.27
Active (*N* = 99)	.63	−.64	−.56
Highly active (*N* = 53)	.73	−.58	−.74
Leaders (*N* = 33)	.73	−.84	−1.14
Earth Island			
Inert (*N* = 24)	.63	−.69	−.40
Active (*N* = 103)	.95	−.78	−.96
Highly active (*N* = 162)	1.60	−1.00	−1.37
Leaders (*N* = 29)	1.87	−1.63	−1.41

Note: Each cultural bias scale runs from −3 (strongly disagree) to +3 (strongly agree).

consensus on issues of deep ecology. While activists generally feel we would be better off with a dramatic population reduction, the public does not. And the core deep ecology tenet that humans are no more important than any other species divides environmentalists, while the public decisively rejects such a notion. In fact, one-third of the Salem-Yamhill sample disagrees in the strongest possible terms. The substantial divergence on questions of deep ecology and technology suggests that beneath broadly shared environmental concerns are often quite different conceptions of how human beings should relate to nature and how environmental problems should be handled.[12]

The consensus thesis fares particularly poorly when we compare the cultural biases of the general public with those of environmental activists.[13] As Table 4 documents, the public is far less egalitarian and far more supportive of both hierarchy and market individualism. Moreover, this cultural gap becomes more pronounced as one moves toward greater activism and leadership. In each of the three environmental groups surveyed, as Table 5 shows, the more active members consistently express greater antipathy to individualism, greater support for egalitarianism, and more distrust of hierarchical authority relations.

THE POLICY IMPLICATIONS OF CULTURAL DISSENSUS

What does the divergence in cultural bias between environmental activists and the general public mean for environmental policymaking? Having pronounced the discovery of a "pervasive and strong" commitment to environmental values among the American public, Kempton et al. are led to ask: "Why is there not more environmental action? Why don't people act collectively to strengthen environmental laws, build infrastructure with reduced environmental impact, or institute incentives for behavior change?" (1995, 220). In other words, why, if Americans share the same basic values as environmentalists, do they not behave more like environmentalists? Kempton et al. explain this puzzle by focusing on the "barriers to action" that prevent widely shared environmental values from being transformed into "environmentally beneficial actions" (p. 220). Among these barriers are misinformation (the mistaken belief, for example, that cutting down rainforests is causing global warming), "structural constraints" (that we lack a strong public transit system or bikeway system, for instance), "fundamental incentive structures" (for instance, that capitalist "companies earn more by producing and selling more, not by making people more satisfied with life"), as well as "divergent individual and group self-interests" (pp. 220, 212).

Our findings of cultural dissensus suggest that the impediments to environmental actions go well beyond institutional constraints, self-interest, or inadequate information. Or, rather, each of these is itself shaped by cultural commitments. Americans do not behave more like environmental activists because culturally they are quite unlike them. To begin with, the general public overwhelmingly believes in markets, business, and capitalism, while the activists generally do not. Most Americans do not see a lot wrong with a system that enables one to acquire more goods rather than being content with only a few. For environmental activists committed to an egalitarian culture, living with less is not just a sacrifice for the sake of the environment but a concrete expression of their commitment to an egalitarian way of life and their rejection of the acquisitive life of competitive individualism. To pose the issue in terms of self-interest versus altruism is to miss the rival value systems that undergird environmental policy debates.

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[12] It is possible that there is an emerging consensus on biocentrism (Kempton, Boster, and Hartley 1995, 224). Such a claim is difficult to test since it involves the future direction of change. It is possible to say, however, that judging by our data, no such consensus has yet emerged. Moreover, to the extent that environmental attitudes remain tied to contested egalitarian and individualistic cultural biases, we would expect that it will be difficult for such a consensus to emerge.

[13] Caution is necessary in generalizing from the Salem-Yamhill sample, which differs in a number of important ways from the nation or even the Pacific Northwest. Tables 4 and 5 likely overstate to some extent the differences in cultural bias between activists and the public. Two surveys by Jenkins-Smith (1994b) in New Mexico and the United States used an item virtually identical to one of ours: "What our country needs is a fairness revolution" (Jenkins-Smith used "society" rather than "country"). He found mean scores of −.14 for the state and −.22 for the nation. While these scores are still slightly less egalitarian than those of Audubon (−.06) and Sierra Club members (.03), they are more egalitarian than for the Salem-Yamhill sample (−.47). For another similar item, "People who are successful in business have a right to enjoy their wealth as they see fit" (Jenkins-Smith's item was, "People who get rich in business have a right to keep and enjoy their wealth"), he found that the public overwhelmingly endorsed this individualistic measure (mean scores of 1.33 and 1.40) in ways that seem comparable to the Salem-Yamhill sample (1.20) and quite unlike our activists. Obviously, larger, national surveys are needed to gauge more accurately the attitudinal gap between activists and the general public.

Our point is not that activists want environmental amenities that the general public does not value—quite the contrary. Both want cleaner air and water, wilderness and species preservation, habitat protection, and a healthier, safer earth. Both are concerned about environmental degradation and are uneasy about the prospect of environmental catastrophe. But the divergence in their cultural biases is important because these biases shape the means activists choose to realize shared environmental goals. More specifically, since many or perhaps most activists do not trust markets, believing it unjust that resource allocations should reflect willingness and ability to pay, they may spurn policies that rely on markets to solve problems. Our evidence suggests that the public may be more willing to accept such market-based solutions to environmental problems.

IMPLICATIONS FOR "CULTURAL THEORY"

The strong relationships reported in this article between egalitarian and antimarket cultural biases, on the one hand, and environmental attitudes and beliefs, on the other, cast serious doubt on Paehlke's ideological autonomy thesis. Our evidence regarding the New Politics thesis is more mixed. Individual-level correlations show little or no relationship between an elite-challenging outlook and environmental attitudes; yet, environmental activists are far more elite-challenging than are the Salem-Yamhill public. The dramatic difference in cultural biases between activists and the public undercuts the cultural consensus thesis, as do the substantial differences in cultural bias between radical and mainstream environmental groups. Our findings, then, refute the autonomy and consensus theses, but do they vindicate Douglas and Wildavsky's cultural theory?

Our results support the claim that environmental attitudes are embedded in broader sociocultural orientations. Moreover, Douglas, Wildavsky, and their various collaborators appear to be correct in arguing that the value one places on equality and markets is an important determinant of environmental attitudes and beliefs. Furthermore, these cultural theorists, together with theorists of the New Politics, appear to be correct in their view that environmental activists often adhere to cultural orientations markedly divergent from those of the general public. Nevertheless, our findings hardly demonstrate the indisputable triumph of cultural theory. Proponents of that theory posit four distinct cultural biases and four distinct attitudes toward nature, yet we found the environmental debate to be primarily a dispute between egalitarians and individualists (cf. Cotgrove 1982). Although we did not test for fatalism, the other cultural bias we tested, hierarchy, proved to be only weakly and inconsistently related to environmental attitudes.

This muddled empirical result mirrors a theoretical muddle about how hierarchical attitudes toward environmental issues are supposed to differ from individualistic attitudes (see especially Dake and Wildavsky 1991, 17). At times, cultural theorists collapse market individualism and hierarchy into a single "establishment" or "center" culture that downplays environmental and technological risks (Douglas and Wildavsky 1982, chapter 5); at other times, they suggest that hierarchists will be somewhere in between the environmental alarmism of egalitarians and the unbridled optimism of competitive individualists (Thompson, Ellis, and Wildavsky 1990, 26–7). Although we found that the hierarchy and individualism scales were intercorrelated,[14] our results are more consistent with the latter than the former hypothesis. The relationship between hierarchy and environmentalism is radically different from that between individualism and environmentalism. Although hierarchal attitudes toward the environment can be distinguished from individualistic attitudes, it is hard to say positively what *is* the hierarchical conception of the environment.

Moreover, we found that ideological self-designation on the liberal-conservative scale correlates highly with both egalitarianism and market individualism, which suggests that these cultural biases are conceptually similar to the conventional left-right ideological continuum (cf. Dake and Wildavsky 1991).[15] Would it not be better, then, to speak of ideology rather than cultural bias? The difference may be largely semantic; indeed, Wildavsky often seems to use the terms "cultural bias" and "ideology" almost interchangeably (1987, 13, and 1988, 593, 594; also see Fine 1995, 130, 132; Wilson 1992, chapter 1). Ideology, as Todd Gitlin has said, is but "contested culture" (Swidler 1986, 279, n14). Wildavsky tends to prefer "culture" to "ideology," however, because, as Geertz (1973, esp. 196–200) pointed out more than thirty years ago, the term "ideology" connotes psychologically "deformed" or

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[14] The Pearson's r correlation coefficient between hierarchy and individualism is .45 in the activist sample and .27 in the general public sample. These intercorrelations, however, are substantially less strong than previous research (using different items) has found. In Dake and Wildavsky (1991, 21), the intercorrelation between market individualism and hierarchy is .54; in Marris, Langford, and O'Riordan (1996, Table 6.2; also see 27) it is .65 or .53, depending on the items used.

[15] For egalitarianism and ideological self-designation, Pearson's r is .52 among activists and .45 among the Salem-Yamhill public. The correlation between market individualism and ideology is −.63 among activists and −.45 among the general public. The correlation between hierarchy and ideology is −.49 in the activist sample but only −.23 among the general public.

"distorted" thought. Wildavsky, following Douglas, prefers the term "cultural bias" because he means to insist that all preferences, not just those with which he disagrees, are "mired in bias" (Wildavsky 1984, 25).

The problem with using "culture" when not referring to entire nations or linguistic, religious, or ethnic groups is that this is at odds with standard usage (Laitin 1988, 589). For Douglas and Wildavsky, however, the key gauge of culture is not a shared language, god, or flag but whether the same people consistently line up on the same side of seemingly unrelated issues (Wildavsky 1988, 593, 595). Our survey did not probe attitudes beyond the environmental domain, but research on new social movements has consistently demonstrated the overlap between the personnel, methods, and ideology of the environmental, peace, antinuclear, feminist, and consumer protection movements (Johnston and Klandermans 1995a; O'Riordan 1995, 5). Douglas and Wildavsky's cultural theory implies that those who are active across a range of social movements will share the same cultural bias or, if you must, ideology or even "ideoculture" (Johnston and Klandermans 1995b, 12).

The suitability of the term "culture" is especially problematic when one relies, as we have, on attitudinal data alone. A full test of the Douglas-Wildavsky cultural theory would incorporate ethnographic data detailing social and institutional relations within environmental groups (Marris, Langford, and O'Riordan 1996, 6; Gross and Rayner 1985; cf. Laitin 1988, 591–2). If we found, for example, that groups whose members share egalitarian preferences (Earth Island and Earth First! in our surveys) are also organized in a more egalitarian manner, then we would be more justified in referring to "culture" rather than "ideology," since shared social relations as well as values and beliefs would be involved.

In an exchange with Wildavsky in the pages of this journal, Laitin objected that Wildavsky's cultural theory "does not answer his question about the source of preferences but merely pushes it back one step" (Laitin 1988, 590; also Friedman 1991, 346). Egalitarianism explains a substantial amount of variation in environmental preferences, but what explains the preference for egalitarianism? This is an important question, but to concede that Wildavsky's cultural theory pushes the question of preference formation back a step is not to deny the utility of cultural explanations (cf. Ellis 1993, 106–7). As a logical matter, there can be *no* value or preference about which one cannot ask a further "but why?" question. Laitin's objection, then, is not specific to the Douglas-Wildavsky version of cultural theory; it applies to all attempts to explain specific beliefs or attitudes in terms of more general and stable values or worldviews.[16] Douglas and Wildavsky freely admit that their cultural theory "has very little to say about people's choices between social forms" (Douglas 1982b, 7; also Wildavsky 1987, 4, 6). What their cultural theory does say is that once an individual opts for a particular set of ideas and institutions, a wide range of other attitudes and behaviors follow. Predicting a lot from a little is our idea of a powerful theory.

APPENDIX A: SAMPLE INFORMATION AND METHODS

Environmental Activists/Members

The Audubon sample was obtained from an August 1994 membership list of the Salem Audubon Society. We selected a systematic sample of 400 names from a list of more than 800. Although the majority of the chapter lives in Salem, members also reside in a number of smaller towns in the area. Of the 400 surveys sent out in May 1995, 36 were returned undeliverable; the 214 surveys returned completed represent a response rate of 59% (214/364). The Sierra Club sample was randomly drawn by club officials from the membership lists of the Oregon chapter, which counts approximately 14,000 members. Surveys were mailed in June 1995. Three were returned undeliverable, and 202 were completed and returned on time, for a response rate of 51% (202/397). The Earth Island sample was drawn by obtaining a list of members in Oregon and Washington. We sent the survey to each of the 246 Oregon members and randomly selected 254 of the 410 Washington members in May 1995. Of these 500 surveys, 16 were returned undelivered; the 290 completed surveys represent a response rate of 60% (290/484). One follow-up mailing was used for each group.

Salem-Yamhill Public

In December 1995, we mailed a shorter version of the survey to residents of Yamhill County and Salem, Oregon. Systematic probability samples of 400 households in the county and 400 households in Salem-Keizer were drawn from the respective phone books. For Salem-Keizer, 24 surveys were undeliverable, and the 197 surveys returned and completed yielded a response rate of 52% (197/376). For Yamhill County, 15 surveys were undeliverable, and the 180 surveys returned and completed represented a response rate of 47% (180/385). The total response rate for the two surveys was 50% (377/761). Two follow-up mailings were made. To compensate for a pronounced gender imbalance (almost two males to every one female), we weighted the sample for gender in calculating mean scores and frequencies for the Salem-Yamhill data.

Demographically, the Salem-Yamhill respondents have substantially lower education levels than do members of the three environmental groups, who tend to be extremely well educated. Almost three-quarters of the environmentalists claim to have college degrees, as compared to 43 percent of the public sample (46% in Salem, 38% in Yamhill). In age and household income, the two samples look remarkably similar, with environmentalists slightly more affluent and no different in average age. Within the environmental groups, however, there was substantial age variation. Audubon respondents are substantially older than either Sierra Club or Earth Island respondents. Among the former, for instance, 36% are over age 65, and only 22% are under age 45, compared to 22% and 43% for the latter two.

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[16] A more troubling question remains, however. According to Wildavsky (1988, 6), what matters most to people is how they live with one another, but how do we know this is true? What matters most to some people could be how they live with nature. It is by no means obvious that cultural bias is primary and environmental commitment secondary or derivative. The strong relationship we uncovered between cultural bias and environmental values and beliefs is consistent with the Wildavsky-Douglas hypothesis, but it is also consistent with a model that portrays people deriving their cultural biases from their ecological concerns. Judging between these alternative models calls for further research into the relative stability of cultural biases and environmental values and beliefs as well as their temporal development.

Environmental Leaders

In late summer 1995, the same version of the environmental survey was sent to 50 leaders in both the Sierra Club and the Earth Island Institute. The latter list was obtained from the projects given on the inside cover of a recent issue of the group's magazine, in consultation with Mary Houghteling, Earth Island's Development Director. The Sierra Club leadership list was compiled by Bob Frenkel, chair of the Oregon chapter. We received 33 surveys from the Sierra Club and 29 from Earth Island, for a response rate of 66% and 58%, respectively. No follow-up letter was sent.

Earth First!

The sample was obtained from Dan Metz's (1995) survey of the Portland-based group, the Voluntary Human Extinction Movement. The group's motto, "May we live long and die out," expresses its leader's belief that only by voluntarily halting human reproduction can the earth and its creatures recover and flourish. The questionnaire, which included a number of the same items used in the environmental survey, was sent in April 1995 to all U.S. subscribers of the group's newsletter, *These Exit Times*. Only two were returned as undeliverable. The response rate, with no follow-up mailing, was 58% (219/375). The survey included a question asking respondents whether they belonged to Earth First! Seventy-nine respondents (36%) reported that they did.

APPENDIX B: MEASUREMENT AND SCALE CONSTRUCTION

For the first seven categories below, reference is made to a seven-point scale: strongly disagree (−3), disagree (−2), somewhat disagree (−1), neither agree nor disagree (0), somewhat agree (+1), agree (+2), strongly agree (+3).

New Ecological Consciousness

The scale is scored as an average of the ten items in Table B-1. Each item was scored on a seven-point scale. Standard deviation and the internal consistency of items are reported in Table B-1.

Environmental Concern

The scale is scored as an average of the five items in Table B-2. Each item was scored on a seven-point scale. Standard deviation and the internal consistency of items are reported in Table B-2. Scoring on items 2 and 5 was reversed.

Egalitarianism

The scale is scored as an average of the three egalitarian items in Table B-3. Each item was scored on a seven-point scale. Standard deviation and the internal consistency of items are reported in Table B-3.

Hierarchy

The scale is scored as an average of the three hierarchy items in Table B-3. Each item was scored on a seven-point scale. Standard deviation and the internal consistency of items are reported in Table B-3.

TABLE B-1. New Ecological Consciousness

	Valid *N*	Standard Deviation	Item-to-Total Correlation	Alpha If Item Deleted
1. If things continue on their present course, we will soon experience a major ecological catastrophe.	704	1.30	.64	.80
2. What human beings are currently doing to nature can be fairly characterized as an "ecoholocaust."	700	1.50	.65	.80
3. Humans are no more important than any other species.	701	2.00	.42	.82
4. We would be better off if we dramatically reduced the number of people on this earth.	697	1.78	.43	.82
5. No wild place will be safe from us until we reconsider our devout belief that economic growth is always good.	700	1.47	.55	.81
6. We can only save the planet by radically transforming our social lives with each other.	698	1.61	.46	.82
7. Unrelenting exploitation of nature has driven us to the brink of ecological collapse.	702	1.51	.60	.80
8. We have reduced natural beauty to postcard prettiness, just another commodity for our consumption.	697	1.63	.58	.80
9. Human happiness and human reproduction are less important than a healthy planet.	694	1.67	.43	.82
10. The oceans are gradually dying of oil pollution and dumping of waste.	703	1.21	.49	.82
Ten-item New Ecological Consciousness Scale	663	.99	NA	NA
Cronbach's alpha = .83				

NA = Not applicable.

TABLE B-2. Environmental Concern

	Valid N	Standard Deviation	Item-to-Total Correlation	Alpha If Item Deleted
1. If things continue on their present course, we will soon experience a major ecological catastrophe.	376	1.88	.80	.82
2. The problems of the environment are not as bad as most people think.	376	1.77	.68	.85
3. The oceans are gradually dying from oil pollution and dumping of waste.	377	1.73	.71	.84
4. We are fast using up the world's natural resources.	375	1.69	.69	.85
5. People worry too much about human progress harming the environment.	377	1.88	.63	.86
Five-item Environmental Concern Scale	373	1.46	NA	NA
Cronbach's alpha = .87				

NA = Not applicable.

TABLE B-3. Cultural Biases

	Sample	Valid N	Standard Deviation	Item-to-Total Correlation	Alpha If Item Deleted
Egalitarianism					
1. The world would be a more peaceful place if its wealth were divided more equally among nations.	Activist	701	1.78	.61	.70
	Public	374	1.85	.55	.75
2. We need to dramatically reduce inequalities between the rich and the poor, whites and people of color, and men and women.	Activist	699	1.49	.60	.71
	Public	370	1.90	.61	.68
3. What our country needs is a fairness revolution to make the distribution of goods more equal.	Activist	695	1.66	.63	.67
	Public	370	1.75	.65	.64
Three-item Egalitarianism Scale	Activist	689	1.37	NA	NA
	Public	368	1.52	NA	NA
Cronbach's alpha = .77 in both populations.					
Hierarchy					
1. One of the problems with people today is that they challenge authority too often.	Activist	700	1.54	.55	.69
	Public	375	1.83	.45	.56
2. Society works best when people strictly obey all rules and regulations.	Activist	700	1.63	.59	.64
	Public	372	1.61	.49	.49
3. Respect for authority is one of the most important things that children should learn.	Activist	703	1.74	.58	.65
	Public	375	1.35	.43	.58
Three-item Hierarchy Scale	Activist	698	1.33	NA	NA
	Public	370	1.23	NA	NA
Cronbach's alpha = .75 in activist sample and .64 in Salem-Yamhill public sample.					
Individualism					
1. Competitive markets are almost always the best way to supply people with the things they need.	Activist	700	1.70	.44	.57
	Public	377	1.50	.41	.59
2. Society would be better off if there was much less government regulation of business.	Activist	701	1.65	.45	.55
	Public	374	1.82	.46	.54
3. People who are successful in business have a right to enjoy their wealth as they see fit.	Activist	700	1.73	.47	.53
	Public	373	1.53	.49	.49
Three-item Individualism Scale	Activist	693	1.30	NA	NA
	Public	372	1.24	NA	NA
Cronbach's alpha = .64 in both populations.					

NA = Not applicable.

Individualism

The scale is scored as an average of the three individualism items in Table B-3. Each item was scored on a seven-point scale. Standard deviation and the internal consistency of items are reported in Table B-3.

Spending on the Environment

Item wording was: "We are spending too little money on improving and protecting the environment." The item was scored on a seven-point scale. In the activist sample, standard deviation = 1.18; in the unweighted Salem-Yamhill sample, standard deviation = 1.87.

Participation

Item wording was: "We should all participate in each decision that directly affects us." The item was scored on a seven-point scale. Standard deviation = 1.49. The item was not used in the Salem-Yamhill survey.

Political Activity

Environmental group members were asked: "Which, if any, of the following activities have you engaged in during the past four years? (1) Written a letter to a member of Congress or the state legislature about an environmental issue. (2) Written a letter to the editor about an environmental issue. (3) Boycotted a company's products because of its record on the environment. (4) Done volunteer work for an environmental group. (5) Attended a meeting of an environmental group." In Table 5 in the text, those who reported performing one or fewer of these activities were scored as "inert"; those performing two or three were scored as "active"; and those performing four or five were scored as "highly active."

Party Identification

"Do you think of yourself as closer to the Republican Party or Democratic Party." Values ranged from 0 (Republican) to 1 (Democrat), with "Neither" as a third option. Standard deviation = .33 in activist sample, .50 in unweighted Salem-Yamhill sample. In the regression analysis reported in text Table 3 and the partial correlations reported in text Table 1, dummy variables were created for Republican and for Democrat.

Ideology

"In general, when it comes to politics, do you usually think of yourself as . . . Strongly Conservative, Conservative, Somewhat Conservative, Somewhat Liberal, Liberal, Strongly Liberal, None of the Above." Standard deviation = 1.17 in activist sample, 1.15 in unweighted Salem-Yamhill sample.

Age

Responses were coded in six categories, from under age 25 (1) to over age 65 (6). Standard deviation = 1.48 in activist sample, 1.40 in unweighted Salem-Yamhill sample.

Education

Responses were coded in six categories, from some high school (1) to graduate degree (6). Standard deviation = 1.37 in activist sample, 1.52 in unweighted Salem-Yamhill public.

Gender

Responses were coded female (0) or male (1). Standard deviation = .50 in activist sample, .47 in unweighted Salem-Yamhill sample.

Household Income

Responses were coded in six categories, from less than $20,000 (1) to more than $100,000 (6). Standard deviation = 1.49 in activist sample, 1.41 in unweighted Salem-Yamhill sample.

APPENDIX C. ROTATED FACTOR MATRICES

	Factor 1	Factor 2
Salem-Yamhill		
Peaceful place	.77343	−.16219
Reduce differences	.78429	−.24657
Fairness revolution	.85439	−.14971
Competitive markets	−.20150	.69237
Less regulation	−.05738	.81803
Enjoy wealth	−.30116	.71093
Environmentalists		
Peaceful place	.79551	−.19512
Reduce differences	.79781	−.20145
Fairness revolution	.82797	−.19571
Competitive markets	−.32272	.64730
Less regulation	−.07675	.81083
Enjoy wealth	−.21482	.74243

Note: In each sample, an initial principal components extraction produced two factors with eigenvalues greater than one. The eigenvalue of the first factor was 2.78 (46.4% of the variance) in the Salem-Yamhill sample and 2.85 (47.6% of the variance) in the environmentalist sample; the eigenvalue of the second factor was 1.06 (17.6% of the variance) in the Salem-Yamhill sample and 1.00 (16.7% of the variance) in the environmentalist sample. Subsequent common factor analysis retaining these two factors, with varimax rotation, yielded the factor loadings reported in this table.

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The Realist Paradigm and Degenerative versus Progressive Research Programs: An Appraisal of Neotraditional Research on Waltz's Balancing Proposition

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Several analysts argue that, despite anomalies, the realist paradigm is dominant because it is more fertile than its rivals. While the ability of the realist paradigm to reformulate its theories in light of criticism accounts for its persistence, it is argued that the proliferation of emendations exposes a degenerating tendency in the paradigm's research program. This article applies Lakatos's criterion that a series of related theories must produce problemshifts that are progressive rather than degenerating to appraise the adequacy of realist-based theories on the balancing of power advanced by neotraditionalists. This research program is seen as degenerating because of (1) the protean character of its theoretical development, (2) an unwillingness to specify what constitutes the true theory, which if falsified would lead to a rejection of the paradigm, (3) a continual adoption of auxiliary propositions to explain away flaws, and (4) a dearth of strong research findings.

Within international relations inquiry, the debate over the adequacy of the realist paradigm has been fairly extensive since the 1970s. In Europe it is often referred to as the interparadigm debate (see Banks 1985; Smith 1995, 18–21). In North America, the focus has been more singularly on realist approaches and their critics (see Vasquez 1983). Toward the end of the 1970s, it appeared that alternate approaches, such as transnational relations and world society perspectives, would supplant the realist paradigm. This did not happen, partly because of the rise of neorealism, especially as embodied in the work of Waltz (1979). Now the debate over the adequacy of the realist paradigm has emerged anew.

In this analysis, *realism* is defined as a set of theories associated with a group of thinkers who emerged just before World War II and who distinguished themselves from idealists (i.e., Wilsonians) on the basis of their belief in the centrality of power for shaping politics, the prevalence of the practices of power politics, and the danger of basing foreign policy on morality or reason rather than interest and power. The *realist paradigm* refers to the shared fundamental assumptions various realist theorists make about the world. Derived primarily from the exemplar of realist scholarship, Morgenthau's ([1948] 1978) *Politics among Nations*, these include: (1) Nation-states are the most important actors in international relations; (2) there is a sharp distinction between domestic and international politics; and (3) international relations is a struggle for power and peace. Understanding how and why that struggle occurs is the major purpose of the discipline (see Vasquez 1983, 15–9, 26–30 for elaboration and justification).

While much of the debate over realism has focused on a comparison to neoliberalism (see Kegley 1995),[1] the debate has also raised new empirical (Rosecrance and Stein 1993), conceptual (Lebow and Risse-Kappen 1995, Wendt 1992), and historical (Schroeder 1994a) challenges to the paradigm as a whole. Some call for a sharp break with the paradigm (e.g., Vasquez 1992), while others see the need to reformulate on the basis of known empirical regularities (Wayman and Diehl 1994). Many still see it as the major theoretical framework within which the field must continue to work (Hollis and Smith 1990, 66), and even critics like Keohane ([1983] 1989) and Nye (1988) see the need to synthesize their approaches (in this case neoliberalism) with the realist paradigm.

If any progress is to be made, scholars must have a set of criteria for appraising the empirical component of theories and paradigms (see Vasquez 1992, 1995). Appraising a paradigm, however, is difficult because often its assumptions are not testable, since they typically do not explain anything in and of themselves (e.g., nation-states are the most important actors). Essentially, a paradigm promises scholars that if they view the world in a particular way, they will successfully understand the subject they are studying. In Kuhn's ([1962] 1970, 23–4) language, paradigms do not so much provide answers as the promise of answers. Ultimately, a paradigm must be appraised by its utility and its ability to make good on its promise. Thus, a paradigm can only be appraised indirectly by examining the ability of the theories it generates to satisfy criteria of adequacy.

Within mainstream international relations, the work of Lakatos (1970) has attracted the most consensus as a source of such criteria among both quantitative and

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[1] "Neoliberalism" is a label employed by a number of scholars (see Nye 1988, 1993, 36–40) to refer to a theoretical approach associated with a cluster of three ideas: (1) Democracies do not fight one another (an idea going back at least to Kant); (2) free trade and growing wealth will create a harmony of interests that will reduce the need for war (the position of the early free traders); and (3) reason can be used to reduce global anarchy and produce more orderly relations among states in part through the creation of global institutions (ideas associated with Grotius and, later, Wilson). For a complete review, see the authors in Kegley 1995; see also Doyle 1986.

traditional scholars (see Keohane [1983] 1989). Although the appraisal of theories and the paradigms from which they are derived involves a number of criteria (see Simowitz and Price 1990), including, in particular, the criterion of empirical accuracy (the ability to pass tests) and the principle of falsifiability, the present analysis will apply only the main criterion on which Lakatos laid great stress for the evaluation of a series of theories: They must produce a progressive as opposed to a degenerating research program. Lakatos's criteria clearly stem from a more positivist perspective, but since realists and neorealists accept them, they are perfectly applicable.[2]

One main difference between Lakatos and early positivists is that Lakatos believes the rules of theory appraisal are community norms and cannot be seen as logically compelling, as Popper (1959) had hoped. The case that any given research program is degenerating (or progressive) cannot be logically proven. Such a stance assumes a foundationalist philosophy of inquiry that has been increasingly under attack in the last two decades (see Hollis and Smith 1990). A more reasonable stance is that exemplified by the trade-off between type 1 and type 2 errors in deciding to accept or reject the null hypothesis. Deciding whether a research program is degenerating involves many individual decisions about where scholars are willing to place their research bets, as well as collective decisions as to which research programs deserve continued funding, publication, and so forth. Some individuals will be willing to take more risks than others. This analysis seeks to present evidence that is relevant to the making of such decisions.

The task of determining whether research programs are progressive or degenerating is of especial importance because a number of analysts (e.g. Hollis and Smith 1990, 66; Wayman and Diehl 1994, 263) argue that, despite anomalies, the realist paradigm is dominant because it is more enlightening and fertile than its rivals. While the ability of the realist paradigm to reformulate its theories in light of conceptual criticism and unexpected events is taken by the above authors as an indicator of its fertility and accounts for its persistence, the proliferation of emendations may not be a healthy sign. Indeed, it can be argued that persistent emendation exposes the degenerating character of the paradigm. This analysis will demonstrate that the "theoretical fertility" apparently exhibited by realism in the last twenty years or so is actually an indicator of the degenerating nature of its research program.

THE CRITERION

Imre Lakatos (1970) argued against Popper (1959) and in favor of Kuhn ([1962] 1970) that no single theory can ever be falsified because auxiliary propositions can be added to account for discrepant evidence. The problem, then, is how to evaluate a *series of theories* that are intellectually related.

A series of theories is exactly what is posing under the general rubrics of realism and neorealism. All these theories share certain fundamental assumptions about how the world works.[3] In Kuhn's ([1962] 1970) language, they constitute a family of theories because they share a paradigm. A *paradigm* can be stipulatively defined as "the fundamental assumptions scholars make about the world they are studying" (Vasquez 1983, 5).[4] Since a paradigm can easily generate a family of theories, Popper's (1959) falsification strategy was seen by Lakatos (1970) as problematic, since one theory can simply be replaced by another in incremental fashion without ever rejecting the shared fundamental assumptions. It was because of this problem that Kuhn's sociological explanation of theoretical change within science was viewed as undermining the standard view in philosophy of science, and it was against Kuhn that Lakatos developed his criteria for appraising a series of theories. To deal with the problem of appraising a series of theories that may share a common paradigm or set of assumptions, Lakatos stipulated that a research program coming out of this core must develop in such a way that theoretical emendations are progressive rather than degenerating.

The main problem with this criterion is that, unless it is applied rigorously, with specific indicators as to what constitutes "progressive" or "degenerating" research programs, it will not provide a basis for settling the debate on the adequacy of the realist paradigm. In an early application of this criterion to structural realism, Keohane ([1983] 1989, 43–4, 52, 55–6, 59), for example, goes back and forth talking about not only the fruitfulness of neorealism but also its incompleteness and the general inability of any international relations theory to satisfy Lakatos's criteria (see also Nye 1988, 243).

Eventually, it would be highly desirable to construct operational indicators of the progressive or degenerating nature of a paradigm's research program. Since these are not available, this analysis will explicitly identify the characteristics that will be used to indicate that a research program is degenerating. Lakatos (1970, 116–7) sees a research program as degenerating if its auxiliary propositions increasingly take on the characteristic of ad hoc explanations that do not produce any novel (theoretical) facts, as well as new empirical content. For Lakatos (p. 116), "no experimental result can ever kill a theory: any theory can be saved from counterinstances either by some auxiliary hypothesis or by a suitable reinterpretation of its terms." Since Lakatos (p. 117) finds this to be the case, he asks: Why not "impose certain standards on the theoretical adjustments by which one is allowed to save a theory?" Adjustments that are acceptable he labels progressive, and those that are not he labels degenerating.

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[2] Vasquez (1995) deals with antifoundationalist postpositivist criticisms of such criteria. On the latter, see Lapid (1989).

[3] Theory is defined here as a set of interrelated propositions purporting to explain behavior; see Vasquez 1992, 835–6. Given this definition, which is noncontroversial, the realist paradigm can have many different theories; see Vasquez 1983, 4–6.

[4] Masterman (1970) has criticized Kuhn for using the concept of paradigm ambiguously. This stipulative definition is meant to overcome this objection, while still capturing the essence of what Kuhn ([1968] 1970, Postscript) was trying to do with the concept.

The key for Lakatos is to evaluate not a single theory but a series of theories linked together. Is each "theoryshift" advancing knowledge, or is it simply a "linguistic device" for saving a theoretical approach?[5] A theoryshift or problemshift is considered (1) theoretically progressive if it theoretically "predicts some novel, hitherto unexpected fact" and (2) empirically progressive if these new predictions are actually corroborated, giving the new theory an excess empirical content over its rival (Lakatos 1970, 118). In order to be considered progressive, a problemshift must be *both* theoretically and empirically progressive—anything short of that is defined (by default) as *degenerating* (p. 118). A degenerating problemshift or research program, then, is characterized by the use of semantic devices that hide the actual content-decreasing nature of the research program through reinterpretation (p. 119). In this way, the new theory or set of theories are really ad hoc explanations intended to save the theory (p. 117).

It should be clear from this inspection of Lakatos's criterion that progressive research programs are evaluated ultimately on the basis of a criterion of accuracy, in that the new explanations must pass empirical testing. If this is the case, then they must in principle be *falsifiable*. The generation of new insights and the ability to produce a number of research tests, consequently, are not indicators of a progressive research program, if *these do not result in new empirical content that has passed empirical tests*.

How can one tell whether a series of theories that come out of a research program is degenerating? First, the movement from T to T' *may* indicate a degenerating tendency if the revision of T involves primarily the introduction of new concepts or some other reformulation that attempts to explain away discrepant evidence. Second, this will be seen as degenerating if this reformulating never points to any novel unexpected facts, by which Lakatos means that T' should tell scholars something about the world other than what was uncovered by the discrepant evidence. Third, if T' does not have any of its new propositions successfully tested or lacks new propositions (other than those offered to explain away discrepant evidence), then it does not have excess empirical content over T, and this can be an indicator of a degenerating tendency in the research program. Fourth, if a research program goes through a number of theoryshifts, all of which have one or more of the above characteristics *and* the end result of these theoryshifts is that collectively the family of theories fields a set of contradictory hypotheses which greatly increase the probability of at least one passing an empirical test, then a research program can be appraised as degenerating.

This fourth indicator is crucial and deserves greater explication. It implies that while some latitude may be permitted for the development of ad hoc explanations, the longer this goes on in the face of discrepant evidence, the greater is the likelihood that scientists are engaged in a research program that is constantly repairing one flawed theory after another without any incremental advancement in the empirical content of these theories. What changes is not what is known about the world, but semantic labels to describe discrepant evidence that the original theory(ies) did not anticipate.

How does one determine whether semantic changes are of this sort or the product of a fruitful theoretical development and new insights? An effect of repeated semantic changes which are not progressive is that they focus almost entirely on trying to deal with experimental outcomes or empirical patterns contrary to the initial predictions of the theory. One consequence is that collectively the paradigm begins to embody contradictory propositions, such as (1) war is likely when power is not balanced and one side is preponderant, and (2) war is likely when power is relatively equal. The development of two or more contradictory propositions increases the probability that at least one of them will pass an empirical test. If a series of theories, all derived from the same paradigm (and claiming a family resemblance, such as by using the same name, e.g., Freudian, Marxist, or realist), predict several competing outcomes as providing support for the paradigm, then this is an example of the fourth indicator. Carried to an extreme, the paradigm could prevent any kind of falsification, because collectively its propositions in effect pose the bet: "Heads, I win; tails, you lose." A research program can be considered blatantly degenerative if one or more of the behaviors predicted is only predicted after the fact.

To be progressive, a theoryshift needs to do more than just explain away the discrepant evidence. It should show how the logic of the original or reformulated theory can account for the discrepant evidence and then delineate how this theoretic can give rise to new propositions and predictions (or observations) that the original theory did not anticipate. The generation of new predictions is necessary because one cannot logically test a theory on the basis of the discrepant evidence that led to the theoryshift in the first place, since the outcome of the statistical test is already known (and therefore cannot be objectively predicted before the fact). The stipulation of new hypotheses that pass empirical testing on some basis other than the discrepant evidence is the minimal logical condition for being progressive. Just *how* fruitful or progressive a theoryshift is, beyond the minimal condition, depends very much on how insightful and/or unexpected the novel facts embodied in the auxiliary hypotheses are deemed to be by scholars within the field. Do they tell scholars things they did not (theoretically) know before?

It should be clear that the criteria of adequacy involve the application of disciplinary norms as to what constitutes progress. The four indicators outlined above provide reasonable and fairly explicit ways to interpret the evidence. Applying them to a body of research should permit a basis for determining whether a research program appears to be on the whole degenerative or progressive.

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[5] Lakatos (1970, 118 n3) notes that by "problemshift" he really means "theoryshift" (i.e., a shift from one specific theory to another) but does not use that word because it "sounds dreadful." Actually, it is much clearer. On the claim that the problemshifts which are degenerating are really just linguistic devices to resolve anomalies in a semantic manner, see Lakatos 1970, 117, 119.

It will be argued that what some see as theoretical enrichment of the realist paradigm is actually a proliferation of emendations that prevent it from being falsified. It will be shown that the realist paradigm has exhibited (1) a protean character in its theoretical development, which plays into (2) an unwillingness to specify what form(s) of the theory constitutes the true theory, which if falsified would lead to a rejection of the paradigm, as well as (3) a continual and persistent adoption of auxiliary propositions to explain away empirical and theoretical flaws that greatly exceed the ability of researchers to test the propositions and (4) a general dearth of strong empirical findings. Each of these four characteristics can be seen as "the facts" that need to be established or denied to make a decision about whether a given research program is degenerating.

THE RESEARCH PROGRAM TO BE ANALYZED

Any paradigm worth its salt will have more than one ongoing research program, so in assessing research programs it is important to select those that focus on a core area of the paradigm and not on areas that are more peripheral or can be easily accommodated by a competing paradigm. It also is important that the research program be fairly well developed both in terms of the number of scholars and the amount of time spent on the program.

If one uses Kuhn's ([1962] 1970) analysis to understand the post–World War II development of the field of international relations, there is a general consensus that the realist paradigm has dominated international relations inquiry within the English-speaking world and that Morgenthau's *Politics among Nations* can be seen as the exemplar of this paradigm (see Vasquez 1983 for a test of this claim; see also Banks 1985; Smith 1995; Olson and Groom 1991; and George 1994). Neorealism can be seen as a further articulation of the realist paradigm along at least two lines. The first, by Waltz (1979), brought the insights of structuralism to bear on realism and for this reason is often referred to as structural realism. For Waltz (1979), structure (specifically the anarchic nature of the international system) is presented as the single most important factor affecting all other behavior. The second by Gilpin (1981), brought to bear some of the insights of political economy with emphasis on the effect of the rise and decline of hegemons on historical change. Both of these efforts have developed research programs. Generally, it is fair to say that Waltz has had more influence on security studies, whereas Gilpin has been primarily influential on questions of international political economy. Since the main concern here is with security, peace, and war, this appraisal will concentrate on the work of scholars who have been influenced by Waltz.

A complete case against the realist paradigm needs to look at other aspects of neorealism and to examine classical realism as well. Elsewhere, the quantitative work guided by classical realism has been evaluated (Vasquez 1983). Gilpin's work on war is best treated in conjunction with the power transition thesis of Organski and Kugler (1980), with which it shares a number of similarities (for an initial appraisal see Vasquez 1993, chapter 3; 1996). So, part of the reason for focusing on Waltz and the research agenda sparked by his analysis is that only so much work can be reviewed in depth in a single article.[6] The more compelling reason is that Waltz's analysis has in fact had a great impact on empirical research. His influence on those who study security questions within international relations in what may be called a neotraditional (i.e., nonquantitative) manner is without equal.

Waltz (1979) centers on two empirical questions: (1) explaining what he considers a fundamental law of international politics, the balancing of power, and (2) delineating the differing effects of bipolarity and multipolarity on system stability. While the latter has recently given rise to some vehement debates about the future of the post–Cold War era (see Mearsheimer 1990, Van Evera 1990/91; see also Kegley and Raymond 1994), it has not yet generated a sustained research program. In contrast, the first area has. The focus of this appraisal will be not so much on Waltz himself as on the neotraditional research program that has taken his proposition on balancing and investigated it empirically. This work is fairly extensive and appears to many to be both cumulative and fruitful. Specifically, the analysis will review the work of Walt (1987) and Schweller (1994) on balancing and bandwagoning, the work of Christensen and Snyder (1990) on "buck-passing" and "chain-ganging," and historical case studies that have uncovered discrepant evidence to see how these works have been treated in the field by proponents of the realist paradigm.

In addition, unlike the work on polarity, that on balancing focuses on a core area for both classical realism and neorealism. It is clearly a central proposition within the paradigm (see Vasquez 1983, 183–94), and concerns with it can be traced back to David Hume and from him to the Ancients in the West, India, and China. Given the prominence of the balance-of-power concept, a research program devoted to investigating Waltz's analysis of the balancing of power, which has attracted widespread attention and is generally well treated in the current literature, cannot fail to pass an examination of whether it is degenerating or progressive without reflecting on the paradigm as a whole—either positively or negatively.

Before beginning this appraisal it is important to keep in mind that the criterion on research programs being progressive is only one of several that can be applied to a paradigm. A full appraisal would involve the application of other criteria, such as accuracy, to all areas of the paradigm. Clearly, such an effort is beyond the scope of this analysis. This article provides only one appraisal, albeit a very important one, of a number that need to be conducted. As other appraisals are completed, more evidence will be acquired to make an overall assessment.

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[6] For reason of space I also do not examine formal models of the balance of power, such as those of Wagner (1986) or Niou, Ordeshook, and Rose (1989).

Likewise, because only the research program on balancing is examined, it can be argued that logically only conclusions about balancing (and not the other aspects of the realist paradigm) can be made. This is a legitimate position to take in that it would be illogical (as well as unfair) to generalize conclusions about one research program to others of the paradigm. Those obviously need to be evaluated separately and appraised on their own merit. They may pass or fail an appraisal based on the criterion of progressivity or on other criteria, such as empirical accuracy or falsifiability. Nevertheless, while this is true, it is just as illogical to assume in the absence of such appraisals that all is well with the other research programs.[7]

In fact, the conclusions of this study are not inconsistent with other recent work which finds fundamental deficiencies in the realist paradigm on other grounds, using different methods and addressing different questions—for example, that by Rosecrance and Stein (1993), who look at the role of domestic politics (cf. Snyder and Jervis 1993); Lebow and Risse-Kappen (1995), who examine realist and nonrealist explanations of the end of the Cold War; and George (1994), who examines the closed nature of realist thinking and its negative effects on the field.

Logically, while this analysis can only draw conclusions about the degeneracy (or progressiveness) of the research program on balancing, the implication of failing or passing this appraisal for the paradigm as a whole is not an irrelevant issue. If Waltz's neorealism is seen as reflecting well on the theoretical robustness and fertility of the realist paradigm (Hollis and Smith 1990, 66), then the failure of a research program meant to test his theory must have some negative effect on the paradigm. The question is how negative. The concluding section will return to this issue, since such matters are more fruitfully discussed in light of specific evidence rather than in the abstract.

THE BALANCING OF POWER: THE GREAT NEW LAW THAT TURNED OUT NOT TO BE SO

One of Waltz's (1979) main purposes was to explain what in his view is a fundamental law of international politics: the balancing of power. Waltz (pp. 5, 6, 9) defines theory as statements that explain laws (i.e., regularities of behavior). For Waltz (p. 117), "whenever agents and agencies are coupled by force and competition rather than authority and law," they exhibit "certain repeated and enduring patterns." These he says have been identified by the tradition of *Realpolitik*. Of these the most central pattern is balance of power, of which he says: "If there is any distinctively political theory of international politics, balance-of-power theory is it" (p. 117). He maintains that a self-help system "stimulates states to behave in ways that tend toward the creation of balances of power" (p. 118) and that "these balances tend to form whether some or all states consciously aim to establish [them]" (p. 119). This law or regularity is what the first six of the nine chapters in *Theory of International Politics* are trying to explain (see, in particular, Waltz 1979, 116–28).

The main problem, of course, is that many scholars, including many realists, such as Morgenthau ([1948] 1978, chapter 14), do not see balancing as the given law Waltz takes it to be. In many ways, raising it to the status of a law dismisses all the extensive criticism that has been made of the concept (Claude 1962; Haas 1953; Morgenthau [1948] 1978, chapter 14) (see Waltz 1979, 50–9, 117, for a review). Likewise, it also sidesteps a great deal of the theoretical and empirical work suggesting that the balance of power, specifically, is not associated with the preservation of peace (Organski 1958; Singer, Bremer, and Stuckey 1972; see also the more recent Bueno de Mesquita 1981; the earlier work is discussed in Waltz 1979, 14–5, 119).

Waltz (1979) avoided contradicting this research by arguing, like Gulick (1955), that a balance of power does not always preserve the peace because it often requires wars to be fought to maintain the balance. What Waltz does here is separate two possible functions of the balance of power—protection of the state in terms of its survival versus the avoidance of war or maintenance of the peace. Waltz does not see the latter as a legitimate prediction of balance-of-power theory. All he requires is that states attempt to balance, not that balancing prevents war.

From the perspective of Kuhn ([1962] 1970, 24, 33–4) one can see Waltz (1979) as articulating a part of the dominant realist paradigm. Waltz is elaborating one of the problems (puzzles as Kuhn [1962] 1970, 36–7, would call them) that Morgenthau left unresolved in *Politics among Nations*; namely, how and why the balance of power can be expected to work and how major a role this concept should play within the paradigm. Waltz's (1979) book can be seen as a theoryshift that places the balance of power in much more positive light than does Morgenthau (cf. 1978, chapter 14). This theoryshift tries to resolve the question of whether the balance is associated with peace by saying that it is not. Waltz, unlike Morgenthau, sees the balance as automatic; it is not the product of a particular leadership's diplomacy but of system structure. The focus on system structure and the identification of "anarchy" are two of the original contributions of Waltz (1979). These can be seen as the introduction of new concepts that bring novel facts into the paradigm. Such a shift appears progressive, but whether it proves to be so turns on whether the predictions made by the explanation can pass empirical testing.

It should come as no surprise, therefore, that the proposition on balancing is the focus of much of the research of younger political scientists influenced by

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[7] I am currently engaged in a project to appraise various aspects of the realist paradigm on a variety of criteria; see Vasquez n.d.

Waltz. Walt, Schweller, Christensen and Snyder, and the historian Schroeder all cite Waltz and consciously address his theoretical proposition on balancing. They also cite and build upon the work of one another; that is, those who discuss bandwagoning cite Walt (e.g., Levy and Barrett 1991, Schweller 1994; those who talk about buckpassing cite Christensen and Snyder, 1990). More fundamentally, they generally are interested (with the exception of Schroeder, who is a critic) in working within the realist paradigm and/or defending it. They differ in terms of how they defend realism. Because they all share certain concepts, are concerned with balancing, and share a view of the world and the general purpose of trying to work within and defend the paradigm, they all can be seen as working on the same general research program. Thus, what they have found and how they have tried to account for their findings provide a good case for appraising the extent to which this particular research program is progressive or degenerating.

Balancing versus Bandwagoning

A passing comment Waltz (1979, 126) makes about his theory is that in anarchic systems (unlike domestic systems), balancing not bandwagoning (a term for which he thanks Stephen Van Evera) is the typical behavior.[8] This is one of the few unambiguous empirical predictions in his theory; Waltz (p. 121) states: "Balance-of-power politics prevail wherever two, and only two, requirements are met: that the order be anarchic and that it be populated by units wishing to survive."

The first major test is conducted by Walt (1987), who looks primarily at the Middle East from 1955 to 1979. He maintains that "balancing is more common than bandwagoning" (Walt 1987, 33). Consistent with Waltz, he argues that, in general, states should not be expected to bandwagon except under certain identifiable conditions (p. 28). Contrary to Waltz, however, he finds that they do not balance power! Instead, he shows that they balance against threat (chapter 5), while recognizing that for many realists, states should balance against power (pp. 18–9, 22–3).[9] He then extends his analysis to East-West relations and shows that if states were really concerned with power, then they would not have allied so extensively with the United States, which had a very overwhelming coalition against the USSR and its allies. Such a coalition was a result not of the power of the USSR but of its perceived threat (pp. 273–81).

Here is a clear falsification of Waltz (in the naive falsification sense of Popper 1959; see Lakatos 1970, 116), but how does Walt deal with this counterevidence or counterinstance, as Lakatos would term it? He takes a very incrementalist position. He explicitly maintains that balance of threat "should be viewed as a refinement of traditional balance of power theory" (Walt 1987, 263). Yet, in what way is this a "refinement" and not an unexpected anomalous finding, given Waltz's prediction? For Morgenthau and Waltz, the greatest source of threat to a state comes from the possible power advantages another state may have over it. In a world that is assumed to be a struggle for power and a self-help system, a state *capable* of making a threat must be guarded against because no one can be assured when it may actualize that potential. Hence, states must balance against power regardless of immediate threat. If, however, power and threat are independent, as they are perceived to be by the states in Walt's sample, then something may be awry in the realist world. The only thing that reduces the anomalous nature of the finding is that it has not been shown to hold for the central system of major states, that is, modern Europe. If it could be demonstrated that the European states balanced threat and not power, then that would be a serious if not devastating blow for neorealism and the paradigm.[10]

As it stands, despite the rhetorical veneer, Walt's findings are consistent with the thrust of other empirical research: The balance of power does not seem to work or produce the patterns that many theorists have expected it to produce. For Walt, it turns out that states balance but not for reasons of power, a rather curious finding for Waltz, but one entirely predictable given the results of previous research that found the balance of power was not significantly related to war and peace (Bueno de Mesquita 1981; see also Vasquez 1983, 183–94).

The degenerating tendency of the research program in this area can be seen in how Walt conceptualizes his findings and in how the field "refines" them further. "Balance of threat" is a felicitous phrase. The very phraseology makes states' behavior appear much more consistent with the larger paradigm than it actually is. It rhetorically captures all the connotations and emotive force of balance of power while changing it only incrementally. It appears as a refinement—insightful and supportive of the paradigm. In doing so, it strips away the anomalous nature and devastating potential of the findings for Waltz's explanation.

This problemshift, however, exhibits all four of the characteristics outlined earlier as indicative of degenerative tendencies within a research program. First, the new concept, "balance of threat," is introduced to explain why states do not balance in the way Waltz theorizes. The balance of threat concept does not appear in Waltz (1979) or in the literature before Walt introduced it in conjunction with his findings. Second, the concept does not point to any novel facts other than the discrepant evidence. Third, therefore this new variant of realism does not have any excess empirical content compared to the original theory, except that it now takes the discrepant evidence and says it supports a new variant of realism.

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[8] For Waltz (1979, 126), bandwagoning is allying with the strongest power, that is, the one capable of establishing hegemony. He maintains that such an alignment will be dangerous to the survival of states. Walt (1987, 17, 21–2) defines the term similarly but introduces the notion of threat: "*Balancing* is defined as allying with others against the prevailing threat; *bandwagoning* refers to alignment with the source of danger" (italics in original).

[9] Walt (1987, 172) concludes: "The main point should be obvious: balance of threat theory is superior to balance of power theory. Examining the impact of several related but distinct sources of threat can provide a more persuasive account of alliance formation than can focusing solely on the distribution of aggregate capabilities."

[10] Schroeder (1994a and b) provides this devastating evidence on Europe (see also Schweller 1994, 89–92).

These three degenerating characteristics open up the possibility that, when both the original balance of power proposition and the new balance of threat proposition (T and T', respectively) are taken as two versions of realism, either behavior can be seen as evidence supporting realist theory (in some form) and hence the realist paradigm or approach in general. Waltz (1979, 121) allows a clear test, because bandwagoning is taken to be the opposite of balancing. Now, Walt splits the concept of balancing into two components, either one of which will support the realist paradigm (because the second is but "a refinement" of balance-of-power theory). From outside the realist paradigm, this appears as a move to dismiss discrepant evidence and explain it away by an ad hoc theoryshift. Such a move is also a degenerating shift on the basis of the fourth indicator, because it reduces the probability that the corpus of realist propositions can be falsified. Before Walt wrote, the set of empirical behavior in which states *could* engage that would be seen as evidence falsifying Waltz's balancing proposition was much broader than it was after Walt wrote.

The danger posed by such theoryshifts can be seen by conducting a mental experiment. Would the following theoretical emendation be regarded as a progressive shift? Let us suppose that the concept of bandwagoning now becomes the focus of empirical research in its own right. Waltz (1979, 126) firmly states: "Balancing not bandwagoning is the behavior induced by the system." (Walt 1987, 32, agrees.) If someone finds bandwagoning to be more frequent, should such a finding be seen as an anomaly for Waltz's T, Walt's T', and the realist paradigm, or simply as the foundation to erect yet another version of realism (T'')? If the latter were to occur, it would demonstrate yet further degeneration of the paradigm's research program and an unwillingness of these researchers to see anything as anomalous for the paradigm as a whole.

By raising the salience of the bandwagoning concept and giving an explanation of it, Walt leaves the door open to the possibility that situations similar to the experiment may occur within the research program. Through this door walks Schweller (1994), who argues in contradiction to Walt that bandwagoning is more common than balancing. From this he weaves "an alternative theory of alliances" that he labels "balance of interests," another felicitous phrase, made even more picturesque by his habit of referring to states as jackals, wolves, lambs, and lions. Schweller (1994, 86) argues that his theory is even more realist than Waltz's, because he bases his analysis on the assumption of the classical realists—states strive for greater power and expansion—and not on security, as Waltz (1979, 126) assumes. Waltz is misled, according to Schweller (1994, 85–8), because of his status-quo bias. If he were to look at things from the perspective of a revisionist state, he would see why they bandwagon: to gain rewards (and presumably power).

Schweller (1994, 89–92), in a cursory review of European history, questions the extent to which states have balanced and argues instead that they mostly bandwagon. To establish this claim, he redefines bandwagoning more broadly than Walt; it is no longer the opposite of balancing (i.e., siding with the actor who poses the greatest threat or has the most power) but simply any attempt to side with the stronger, especially for opportunistic gain. Because the stronger state often does not pose a direct threat to every weak state, this kind of behavior is much more common and distinct from what Walt meant.

Two things about Schweller (1994) are important for the appraisal of this research program. First, despite the vehemence of his attack on the balancing proposition, this is nowhere seen as a deficiency of the realist paradigm; rather, it is Waltz's distortion of classical realism (however, see Morgenthau [1948] 1978, 194). The latter is technically true, in that Waltz raises the idea of balancing to the status of a law, but one would think that the absence of balancing in world politics, especially in European history, would have some negative effect on the realist view of the world. Certainly, Schweller's "finding" that bandwagoning is more prevalent than balancing is something classical realists, such as Morgenthau ([1948] 1978), Dehio (1961), or Kissinger (1994, 20–1, 67–8, 166–7) would find very disturbing. They would not expect this to be the typical behavior of states, and if it did occur, they would see it as a failure to follow a rational foreign policy and/or to pursue a prudent realist course (see Morgenthau [1948] 1978, 7–8).

Second, and more important, Schweller's theoryshift (T'') has made bandwagoning a "confirming" piece of evidence for the realist paradigm. So, if he turns out to be correct, his theory, which he says is even more realist than Waltz's, will be confirmed. If he is incorrect, then Waltz's version of realism will be confirmed. Under what circumstances will the realist paradigm be considered as having failed to pass an empirical test? The field is now in a position (in this research program) where any one of the following can be taken as evidence supporting the realist paradigm: balancing of power, balancing of threat, and bandwagoning. At the same time, the paradigm as a whole has failed to specify what evidence will be accepted as falsifying it—a clear violation of Popper's (1959) principle of falsifiability. Findings revealing the absence of balancing of power and the presence of balancing of threat or bandwagoning are taken by these researchers as supporting the realist paradigm; instead, from the perspective of those outside the paradigm, these outcomes should be taken as anomalies. All their new concepts do is try to hide the anomaly through semantic labeling (see Lakatos 1970, 117, 119). Each emendation tries to salvage something but does so by moving farther and farther away from the original concept. Thus, Waltz moves from the idea of a balance of power to simply balancing power, even if it does not prevent war. Walt finds that states do not balance power but oppose threats to themselves. Schweller argues that states do not balance against the stronger but more frequently bandwagon with it to take advantage of opportunities to gain rewards.

Walt and Schweller recognize discrepant evidence and explain it away by using a balance phraseology that hides the fact the observed behavior is fundamentally different from that expected by the original theory. The field hardly needs realism to tell it that states will oppose threats to themselves (if they can) or that revisionist states will seize opportunities to gain rewards (especially if the risks are low). In addition, these new concepts do not point to any novel theoretical facts; they are not used to describe or predict any pattern or behavior other than the discrepant patterns that undercut the original theory.

Ultimately, under the fourth indicator, such theory-shifts are also degenerating because they increase the probability that the realist paradigm will pass some test, since three kinds of behavior now can be seen as confirmatory. While any one version of realism (balance of power, balancing power, balance of threats, balance of interests) may be falsified, the paradigm itself will live on and, indeed, be seen as theoretically robust. In fact, the protean character of realism prevents the paradigm from being falsified because as soon as one theoretical variant is discarded, another variant pops up to replace it as the "true realism" or the "new realism."

The point is not that Walt or others are engaged in "bad" scholarship or have made mistakes; indeed, just the opposite is the case: They are practicing the discipline the way the dominant paradigm leads them to practice it. They are theoretically articulating the paradigm in a normal science fashion, solving puzzles, engaging the historical record, and coming up with new insights—all derived from neorealism's exemplar and the paradigm from which it is derived. In doing so, however, these individual decisions reflect a collective degeneration.

Even as it is, other research on bandwagoning (narrowly defined) has opened up further anomalies for the realist paradigm by suggesting that a main reason for bandwagoning (and indeed for making alliances in general) may not be the structure of the international system but domestic political considerations. Larson (1991, 86–7) argues antithetically to realism that states in a similar position in the international system and with similar relative capabilities behave differently with regard to bandwagoning; therefore, there must be some intervening variable to explain the difference. On the basis of a comparison of cases, she argues that some elites bandwagon to preserve their domestic rule (see also Strauss 1991, 245, who sees domestic considerations and cultural conceptions of world politics as critical intervening variables). Similarly, Levy and Barnett (1991, 1992) present evidence on Egypt and Third World states showing that internal needs and domestic political concerns are often more important in alliance making than are external threats. This research suggests that realist assumptions—the primacy of the international struggle for power and the unitary rational nature of the state will lead elites to formulate foreign policy strictly in accord with the national interest defined in terms of power are flawed. Theories need to take greater cognizance of the role domestic concerns play in shaping foreign policy objectives. To the extent bandwagoning is a "novel" fact (even if not a predominant pattern), it points us away from the dominant paradigm, not back to its classical formulation.

Buck-passing and Chain-ganging

The bandwagoning research program is not the only way in which the protean character of realism has been revealed. Another and perhaps even more powerful example is the way in which Christensen and Snyder (1990) have dealt with the failure of states to balance. They begin by criticizing Waltz for being too parsimonious and making indeterminate predictions about balancing under multipolarity. They then seek to correct this defect within realism, by specifying that states will engage in chain-ganging or buck-passing depending on the perceived balance between offense and defense. Chain-ganging occurs when states, especially strong states, commit "themselves unconditionally to reckless allies whose survival is seen to be indispensable to the maintenance of the balance"; buck-passing is a failure to balance and reliance on "third parties to bear the costs of stopping a rising hegemon" (Christensen and Snyder 1990, 138). The alliance pattern that led to World War I is given as an example of chain-ganging, and Europe in the 1930s is given as an example of buck-passing. The propositions are applied only to multipolarity; in bipolarity, balancing is seen as unproblematic.

This article is another example of how the realist paradigm (since Waltz) has been articulated in a normal science fashion. The authors find a gap in Waltz's explanation and try to correct it by bringing in a variable from Jervis (1978; see also Van Evera 1984). This gives the impression of cumulation and progress through further specification, especially since they have come up with a fancy title for labeling what Waltz identified as possible sources of instability in multipolarity.

A closer inspection reveals the degenerating character of their emendation. The argument that states will either engage in buck-passing or chain-ganging under multipolarity is an admission that in important instances, such as the 1930s, states fail to balance the way Waltz (1979) says they must because of the system's structure. Recall Waltz's (1979, 121) clear prediction that "balance-of-power politics will prevail wherever two, and only two, requirements are met: anarchy and units wishing to survive." Surely, these requirements were met in the period before World War II, and therefore failure to balance should be taken as falsifying evidence.

Christensen and Snyder (1990) seem to want to explain away the 1930s, in which they argue there was a great deal of buck-passing. Waltz (1979, 164–5, 167), however, never says that states will not conform overall) to the law of balancing in multipolarity, only that there are more "difficulties" in doing so. If Christensen and Snyder see the 1930s as a failure to balance properly, then this is an anomaly that needs to be explained away. The buck-passing/chain-ganging concept does that in a rhetorical flourish that grabs attention and seems persuasive. Yet, it "rescues" the theory not simply from indeterminate predictions, as Christensen and Snyder (1990, 146) put it, but explains away a critical case that the theory should have predicted.

This seems to be especially important because, contrary to what Waltz and Christensen and Snyder postulate, balancing through alliances should be more feasible under multipolarity than bipolarity, because under the latter there simply are not any other major states with whom to align. Thus, Waltz (1979, 168) says that under bipolarity *internal* balancing is more predominant and precise than external balancing. If under bipolarity there is, according to Waltz, a tendency to balance (internally, i.e., through military buildups), and under multipolarity there is, according to Christensen and Snyder, a tendency to pass the buck or chain-gang, then when exactly do we get the kind of alliance balancing that we attribute to the traditional balance of power Waltz has decreed as a law? Christensen and Snyder's analysis appears as a "protean-shift" in realism that permits the paradigm to be confirmed if states balance (internally or externally), chain-gang, or buck-pass (as well as bandwagon, see Schweller 1994). This is degenerative under the fourth indicator because the probability of falsification decreases to a very low level. It seems to increase greatly the probability that empirical tests will be passed by some form of realism.[11]

Imprecise measurement leaving open the possibility for ad hoc interpretation is also a problem with identifying buck-passing and chain-ganging. Were Britain, France, and the USSR passing the buck in the late 1930s, or were they just slow to balance? Or were Britain and France pursuing an entirely different strategy, appeasement, because of the lessons they derived from World War I? If the latter, which seems more plausible, then buck-passing is not involved at all, and the factor explaining alliance behavior is not multipolarity but an entirely different variable (see Rosecrance and Steiner 1993). What is even more troubling is that while Christensen and Snyder (1990) see pre-1939 as buck-passing and pre-1914 as chain-ganging, it seems that Britain was much more hesitant to enter the war in 1914 than in 1939, contrary to what one would expect given the logic of Christensen and Snyder's historical analysis.[12] After Hitler took Prague in March 1939, domestic public and elite opinion moved toward a commitment to war (Rosecrance and Steiner 1993, 140), but in 1914 that commitment never came before the outbreak of hostilities (see Levy 1990/91). The cabinet was split, and only the violation of Belgium tipped the balance. Thus, the introduction of the new refinement is far from a clear or unproblematic solution to the anomaly on its own terms.

The refinements of Waltz produced by the literature on bandwagoning and buck-passing are degenerating because they hide, rather than deal directly with, the seriousness of the anomalies they are trying to handle. A theory whose main purpose is to explain balancing cannot stand if balancing is not the law it says it is. Such an anomaly also reflects negatively on the paradigm as a whole. Even though Morgenthau ([1948] 1978, chapter 14) did not think the balance of power was very workable, power variables are part of the central core of his work, and he does say that the balance of power is "a natural and inevitable outgrowth of the struggle of power" and "a protective device of an alliance of nations, anxious for their independence, against another nation's designs for world domination" (Morgenthau [1948] 1978, 194, and see 173, 195–6). Waltz's (1979) theory, which has been characterized as a systematization of classical realism (Keohane 1986, 15) and widely seen as such, cannot fail on one of its few concrete predictions without reflecting badly (in some sense) on the larger paradigm in which it is embedded.

Historical Case Studies

Unlike the explicitly sympathetic work cited above, several historical case studies that focus on the balancing hypothesis give rise to more severe criticism of realist theory. Rosecrance and Stein (1993, 7) see the balancing proposition as the key prediction of structural realism. In a series of case studies, they challenge the idea that balancing power actually occurs or explains very much of the grand strategy of the twentieth-century major states they examine; to explain grand strategy for them requires examining domestic politics (Rosecrance and Stein 1993, 10, 17–21). In contradiction to structural realism, they find that balance-of-power concerns do not take "precedence over domestic factors or restraints" (Rosecrance and Stein 1993, 17). Britain in 1938, the United States in 1940, and even the Soviet Union facing Reagan in 1985 fail to meet powerful external challenges, in part because of domestic political factors (Rosecrance and Stein 1993, 18, and see the related case studies in chapters 5–7). States sometimes under- or overbalance. As Rosecrance (1995, 145) maintains, states rarely get it right—they either commit too much or too little, or they become so concerned with the periphery that they overlook what is happening to the core (see Kupchan 1994, Thompson and Zuk 1986). And, of course, they do this because they are not the unitary rational actors the realist paradigm thinks they are. Contrary to Waltz, and even Morgenthau, states engage in much more variegated behavior than the realist paradigm suggests.

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[11] Of course, one may argue that Christensen and Snyder's (1994) proposition on offense-defense is falsifiable in principle, and that is true, but this points out another problem with their analysis; namely, Levy (1984) is unable to distinguish in specific historical periods whether offense or defense has the advantage (see Christensen and Snyder 1990, 139, 6 and 7). They, in turn, rely on the perception of offense and defense, but such a "belief" variable takes us away from realism and toward a more psychological-cognitive paradigm.

[12] Christensen and Snyder (1990, 156) recognize British buck-passing in 1914, but they say Britain was an outlier and "did not entirely pass the buck."

This last point is demonstrated even more forcibly by the historian Paul Schroeder (1994a and b). He shows that the basic generalizations of Waltz—that anarchy leads states to balancing and to act on the basis of their power position—are not principles that tell the "real story" of what happened from 1648 to 1945. He demonstrates that states do not balance in a law-like manner but deal with threat in a variety of ways; among others, they hide, they join the stronger side, they try to "transcend" the problem, or they balance. In a brief but systematic review of the major conflicts in the modern period, he shows that in the Napoleonic wars, Crimean War, World War I, and World War II there was no real balancing of an alleged hegemonic threat—so much for the claim that this kind of balancing is a fundamental law of international politics. When states do resist, as they did with Napoleon, it is because they have been attacked and have no choice: "They resisted because France kept on attacking them" (Schroeder 1994a, 135; see also Schweller 1994, 92). A similar point also could be made about French, British, Soviet, and American resistance to Hitler and Japan.

Basically, Schroeder shows that the historical record in Europe does not conform to neorealists' theoretical expectations about balancing power. Their main generalizations are simply wrong. For instance, Schroeder does not see balancing against Napoleon, the prime instance in European history in which it should have occurred (see also Rosecrance and Lo 1996). Many states left the First Coalition against revolutionary France after 1793, when they should not have, given France's new power potential. Periodically, states bandwagoned with France, especially after victories, as in late 1799, when the Second Coalition collapsed. According to Schroeder (1994a, 120–1), hiding or bandwagoning, not balancing, was the main response to the Napoleonic hegemonic threat, the exact opposite of the assertions not only by Waltz but also by such long-time classical realists as Dehio (1961). For World War I, Schroeder (1994a, 122–3) argues that the balancing versus bidding for hegemony conceptualization simply does not make much sense of what each side was doing in trying to deal with security problems. With World War II, Schroeder (1994a, 123–4) sees a failure of Britain and France to balance and sees many states trying to hide or bandwagon.[13]

For Schroeder (1994a, 115, 116), neorealist theory is a misleading guide to inquiry:

> The more one examines Waltz's historical generalizations about the conduct of international politics throughout history with the aid of the historian's knowledge of the actual course of history, the more doubtful—in fact, strange—these generalizations become.... I cannot construct a history of the European states system from 1648 to 1945 based on the generalization that most unit actors within that system responded to crucial threats to their security and independence by resorting to self-help, as defined above. In the majority of instances this just did not happen.

All this suggests that the balancing of power was never the law Waltz thought it was. In effect, he offered an explanation of a behavioral regularity that never existed, except within the logic of the theory. As Schroeder (1994b, 147) concludes:

> [My point has been] to show how a normal, standard understanding of neo-realist theory, applied precisely to the historical era where it should fit best, gets the motives, the process, the patterns, and the broad outcomes of international history wrong . . . it prescribes and predicts a determinate order for history without having adequately checked this against the historical evidence.

SHIRKING THE EVIDENCE AND PROVING THE POINT

How have scholars sympathetic to realism responded to Schroeder? They have sought to deny everything and done so precisely in the degenerating manner that Lakatos (1970, 116–9) predicted. The reaction by Elman and Elman (1995) to Schroeder in the correspondence section of *International Security* illustrates best the extent to which the last ten years of realist research have cumulated in degenerating problemshifts. Elman and Elman (1995) make three points against Schroeder (1994a). First, although his evidence may challenge Waltz's particular theory, it still leaves the larger neorealist approach unscathed. Second, Waltz recognizes balancing failures so that not every instance of these necessarily disconfirms his theory. Third, even if Schroeder's evidence on balancing poses a problem for Waltz, "only better theories can displace theories.... Thus, Waltz's theory should not be discarded until something better comes along to replace it" (Elman and Elman 1995, 192).

The first point somewhat misses the mark, since so much of neorealism is associated with Waltz. There remains mostly Gilpin (1981) and Krasner (1978). It is primarily Gilpin whom Elman and Elman have in mind when they argue that Schroeder's "omission of entire neo-realist literatures" leads him to fail to understand that "balancing is not the only strategy which is logically compatible with neo-realist assumptions of anarchy and self-help" (Elman and Elman 1995, 185, 186; see also Schweller 1992, 267, whom they cite).[14] They argue that for Gilpin (1981) and power transition theory "balancing is not considered a prevalent strategy, nor are balances predicted to occur repeatedly" (Elman and Elman 1995, 186). The problem with using Gilpin and the more quantitatively oriented power transition thesis of Organski and Kugler (1980) is that the two main pillars of neorealism predict contradictory things. Thus, between Waltz and Gilpin, threat can be handled by either balancing or not balancing. It certainly is not a very strong defense of neorealism to say that opposite behaviors are both logically compatible with the assumptions of anarchy.

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[13] Numerous other deviant cases are listed in Schroeder (1994a, 118–22, 126–9, 133–47).

[14] By saying that Schroeder leaves much of the neorealist approach unscathed, Elman and Elman (1995) seem to fall into the trap of assuming that Gilpin (1981) is empirically accurate unless proven otherwise. In fact, as related to security questions, Gilpin (1981) has not been extensively tested, and existing tests are not very encouraging (see Spiezio 1990, as well as Boswell and Sweat (1991) and the discussion in Vasquez 1993, 93–8).

The Elmans are technically correct that evidence against balancing does not speak against all the larger realist paradigm in that neorealism also embodies Gilpin. But it is this very correctness that proves the larger point being made here and illustrates what so worried Lakatos about degenerating research programs. At the beginning of this article, four indicators of a degenerating research program were presented. Elman and Elman (1995) serves as evidence that all these are very much in play within the field. On the basis of their defense of neorealism and the review of the literature above, it will be shown that the protean nature of realism, promulgated by the proliferation of auxiliary hypotheses to explain away discrepant evidence, has produced an unwillingness to specify what evidence would in principle lead to a rejection of the paradigm. The result has been a continual theoretical articulation but in the context of a persistent dearth of strong empirical findings.

Using Gilpin and power transition in the manner of the Elmans is degenerating because permitting the paradigm to be supported by instances of either "balancing" or "not balancing" reduces greatly the probability of finding any discrepant evidence. As if this were not enough to cover all sides of the bet, Elman and Elman (1995, 187–8) maintain that, within the neorealist assumption of self-help, threat can be handled by bandwagoning, expansion, preventive war, balancing, hiding, and even what Schroeder has labeled "transcending."[15] In other words, there is always some behavior (in dealing with threat) that will prove realism correct, even though most versions will be shown to be incorrect, and even though neorealists "often consider balancing to be the most successful strategy for most states most of the time" (Elman and Elman 1995, 187). But if this caveat is the case, then why do states not regularly engage in this behavior? Elman and Elman rightly capture the theoretical robustness of the realist paradigm—showing that Waltz, Gilpin, and others are part of the paradigm—but they fail to realize the damning protean portrayal they give of its research program and how this very theoretical development makes it difficult for the paradigm to satisfy the criterion of falsifiability.

Instead, they conclude about Schroeder's (1994a) historical evidence that "no evidence could be more compatible with a neo-realist reading of international relations" (Elman and Elman 1995, 184). They conclude this because each of these strategies (bandwagoning, etc.) does not challenge the realist conception of a rational actor behaving in a situation of competition and opportunity. For them, so long as states choose strategies that are "consistent with their position in the global power structure and pursue policies that are likely to provide them with greater benefits than costs" (Elman and Elman 1995, 184), then this is seen as evidence supporting the broad realist approach. Only Wendt's (1992) claim that states could be "other-regarding" as opposed to "self-regarding" is seen as discrepant evidence (see also Elman 1996, Appendix, Diagram 1). Basically, these are "sucker bets" of the "I win, you lose" variety. Let it be noted that these are not bets that Elman and Elman are proposing; they are merely reporting what, in effect, the entire realist research program has been doing—from Walt, to Christensen and Snyder, to Schweller, and so forth. Collectively, the realist mainstream has set up a situation that provides a very narrow empirical base on which to falsify the paradigm.

What kinds of political actors would, for example, consciously pursue policies that are "likely to provide" them with greater costs than benefits? To see only "other-regarding" behavior as falsifying leaves a rather vast and variegated stream of behaviors as supportive of the paradigm. Schroeder (1995, 194) has a legitimate complaint when he says, in reply: "The Elman argument . . . appropriates every possible tenable position in IR theory and history for the neo-realist camp." He concludes: "Their whole case that history fits the neo-realist paradigm falls to the ground because they fail to see that it is their neo-realist assumptions, as they understand and use them, which simply put all state action in the state system into a neo-realist mold and neo-realist boxes, *by definition*" (Schroeder 1995, 194, emphasis in the original).

Instead of defending the paradigm, Elman and Elman (1995) expose the degenerating nature of its research program and the field's collective shirking of the evidence through proteanshifts. Many neotraditionalists, such as Mearsheimer (1990), have eschewed quantitative evidence challenging the adequacy of the realist paradigm; if realists will now refuse to accept historical evidence as well, then what kind of evidence will they accept as falsifying their theories? Only "other-regarding" behavior? That simply will not do.

The cause of this problem is the lack of rigor in the field in appraising theories. The nature of the problem can be seen in Elman and Elman's (1995) second point against Schroeder. Drawing upon Christensen and Snyder (1990), they note that balancing under multipolarity, for Waltz, is more difficult than balancing under bipolarity: "Thus Schroeder's finding that states failed to balance prior to World War I (pp. 122–3) and World War II (pp. 123–4) does not disconfirm Waltz's argument. . . . In short, a failure to balance is not a failure of balance of power theory if systemic conditions are likely to generate this sort of outcome in the first place" (Elman and Elman 1995, 190–1). This sets up a situation in which any failure to balance under multipolarity can be taken as confirmatory evidence because, according to Elman and Elman (1995, 90), "Waltz's theory also predicts balancing *failures*" (emphasis in the original). This again poses an "I win, you lose" bet. If the periods before World War I and World War II are not legitimate tests of Waltz's prediction of balancing, then what would be? The implication is that balancing more easily occurs under bipolarity, but here external balancing is structurally impossible by definition. If this is the case, how is balancing a "law," or the main outcome of anarchy? This is especially problematic because there is a tendency in Waltz to see only the post-1945 period as a true bipolarity (see Nye 1988, 244), which means the rest of history is multipolar and subject to balancing failures.

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[15] Transcending is seen by Schroeder (1994a) as particularly discrepant for realism, but Elman and Elman (1995, 188) view it as part of the realist approach.

In the end, Elman and Elman (1995, 192) concede that Waltz does believe that, "on aggregate," states should balance, so "Schroeder's evidence that states rarely balance does indeed pose a problem for Waltz's theory." They conclude, however, by citing Lakatos—only better theories can displace theories—and therefore Waltz's theory should not be discarded until something better comes along. They then outline a general strategy for improving the theory, namely, adding variables, identifying the domain to which it is applicable, and broadening definitions (especially of threat). All these, however, are precisely the tactics that have produced the degenerating situation the field now faces. Thus, they say, by broadening the definition of threat to include internal threats from domestic rivals, decision makers could still be seen as balancing, and bandwagoning "would not necessarily disconfirm the prediction that balancing is more common" (Elman and Elman 1995, 192). This would take the discrepant evidence of Levy and Barnett (1991, 1992) and of Larson (1991) and make it confirmatory. This is precisely the kind of strategy that Lakatos (1970, 117–9) decried.

What is also evident from this appraisal of the realist paradigm is that Lakatos's (1970, 119) comment that "there is no falsification before the emergence of a better theory" can play an important role in muting the implications of a degenerating research program, especially when alternative paradigms or competing midrange theories are ignored, as has been the case in international relations. There have been too many empirical failures and anomalies, and theoretical emendations have taken on an entirely too ad hoc nonfalsifying character for adherents to say that the paradigm cannot be displaced until there is a clearly better theory available. Such a position makes collective inertia work to the advantage of the dominant paradigm and makes the field less rather than more rigorous.

If one wants to take the very cautious position that Schroeder's historical evidence affects only Waltz, one should not then be incautious and assume that other research programs within the realist paradigm are doing fine. A more consistent position would be to hold this conclusion in abeyance until all aspects of the paradigm are appraised. The lesson from Schroeder's (1994a and b) discrepant evidence should *not* be that his "article leaves the general neo-realist paradigm unscathed" (Elman and Elman 1995, 192), but that a major proposition of the paradigm has failed to pass an important historical test.

WHERE DO WE GO FROM HERE?

It seems that the internal logic of the Lakatos rules requires that a warning flag on the degenerating direction of the research program on balancing be raised. Theorists should be aware of the pitfalls of setting up realist variants that produce a "heads, I win; tails, you lose" situation, which makes realism nonfalsifiable. In addition, greater efforts need to be made in specifying testable differences between realist and nonrealist explanations before the evidence is assessed, so as to limit the use of ex post facto argumentation that tries to explain away discrepant evidence.

If one accepts the general thrust of the analysis that the neotraditional research program on balancing has been degenerating, then the question that needs to be discussed further is the implications of this for the wider paradigm. Two obvious conclusions are possible. A narrow and more conservative conclusion would try to preserve as much of the dominant paradigm as possible in face of discrepant evidence. A broader and more radical conclusion would take failure in this one research program as consistent with the assessments of other studies and thus as an indicator of a deeper, broader problem. It is not really necessary that one conclusion rather than the other be taken by the entire field, since what is at stake here are the research bets individuals are willing to take with their own time and effort. In this light, it is only necessary to outline the implications of the two different conclusions.

The narrow conclusion is that Waltz's attempt to explain what he regards as the major behavioral regularity of international politics was premature because states simply do not engage in balancing with the kind of regularity that he assumes. It is the failure of neotraditional researchers and historians to establish clearly the empirical accuracy of Waltz's balancing proposition that so hurts his theory. If the logical connection between anarchy (as a systemic structure) and balancing is what Waltz claims it to be, and states do not engage in balancing, then this empirical anomaly must indicate some theoretical deficiency.

The neotraditional approach to date has muted the implications of the evidence by bringing to bear new concepts. The argument presented here is that such changes are primarily semantic and more clearly conform to what Lakatos calls degenerating theoryshifts than to progressive theoryshifts. If this is accepted, then at minimum one would draw the narrow conservative conclusion that the discrepant evidence (until further research demonstrates otherwise) is showing that states do not balance in the way Waltz assumes they do. Realists then can concentrate on other research programs within the paradigm without being susceptible (at least on the basis of this analysis) to the charge of engaging in a degenerating research program. Those who continue to mine realist inquiry, however, should pay more attention to the problem of degeneration in making theoretical reformulations of realism. Specifically, scholars making theoryshifts in realism should take care to ensure that these are not just proteanshifts.

The implication of the broader and more radical conclusion is to ask why a concept so long associated with realism should do so poorly and so misguide so many theorists. Could not its failure to pass neotraditional and historical "testing" (or investigation) be an indicator of the distorted view of world politics that the paradigm imposes on scholars? Such questions are reasonable to ask, especially in light of appraisals that have found other aspects of realism wanting (see Lebow and Risse-Kappen 1995, Rosecrance and Stein 1993, Vasquez 1983), but they are not the same as logically compelling conclusions that can be derived from the analysis herein. It has been shown only that one major research program, which has commanded a great deal of interest, seems to be exhibiting a degenerating tendency.

Such a demonstration is important in its own right, particularly if analysts are unaware of the collective effect of their individual decisions. In addition, it shows that what admirers of the realist paradigm have often taken as theoretical fertility and a continuing ability to provide new insights is not that at all, but a degenerating process of reformulating itself in light of discrepant evidence.

Regardless of whether a narrow or broad conclusion is accepted, this analysis has shown that the field needs much more rigor in the interparadigm debate. Only by being more rigorous both in testing the dominant paradigm and in building a new one that can explain the growing body of counterevidence as well as produce new nonobvious findings of its own will progress be made.

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Evaluating Theories

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John Vasquez claims to follow Imre Lakatos but distorts his criteria for judging theories and evaluating research programs. Vasquez claims that facts observed can falsify a theory by showing that its predictions are wrong. He fails to consider the puzzles posed by the interdependence of theory and fact. He places all realists in a single paradigm despite the divergent assumptions of traditional and structural realists. In contrast to Vasquez, I argue that explanation, not prediction, is the ultimate criterion of good theory, that a theory can be validated only by working back and forth between its implications and an uncertain state of affairs that we take to be the reality against which theory is tested, and that the results of tests are always problematic.

Having previously covered the criticisms John Vasquez makes (see especially Waltz 1979, 1986), I respond to his article reluctantly. One is, however, always tempted to try again.

Following Lakatos (1970), albeit shakily, in moving from paradigms to theories to research programs, Vasquez says he places theories in a single paradigm if they "share certain fundamental assumptions" (p. 900). He thereupon lumps old and new realists together in one realist paradigm. This is odd since, as he recognizes, old and new realists work from different basic assumptions. Believing that states strive for ever more power, Hans Morgenthau took power to be an end in itself. In contrast, I built structural theory on the assumption that survival is the goal of states and that power is one of the means to that end. Political scientists generally work from two different paradigms: one behavioral, the other systemic. Old realists see causes as running directly from states to the outcomes their actions produce. New realists see states forming a structure by their interactions and then being strongly affected by the structure their interactions have formed. Old realists account for political outcomes mainly by analyzing differences among states; new realists show why states tend to become like units as they try to coexist in a self-help system, with behaviors and outcomes explained by differences in the positions of states as well as by their internal characteristics (see Waltz 1990). If the term "paradigm" means anything at all, it cannot accommodate such fundamental differences.

Vasquez puts old and new realists in the same pot because he misunderstands realists. He makes odd statements about what paradigms do because he misunderstands paradigms. He believes that paradigms easily generate a family of theories (p. 900). Paradigms are apparently like sausage machines: Turn the crank, and theories come out. Yet no one in any field is able to generate theories easily or even to say how to go about creating them.

Vasquez finds lots of realist theories because he defines theories loosely as "inter-related propositions purporting to explain behavior" (footnote 3). If interrelating propositions were all it took to make theories, then, of course, we would have many of them. I can, however, think of any number of propositions purporting to explain something that would not qualify as theories by any useful definition of the term. I define theory as a picture, mentally formed, of a bounded realm or domain of activity. A theory depicts the organization of a realm and the connections among its parts. The infinite materials of any realm can be organized in endlessly different ways. Reality is complex; theory is simple. By simplification, theories lay bare the essential elements in play and indicate necessary relations of cause and interdependency—or suggest where to look for them (see Waltz 1979, 1–13). Vasquez, following his definition, finds many theories; I find few.

Vasquez's belief that theories are plentiful and easy to produce reflects the positivist tradition that permeates American political science. At the extreme, positivists believe that reality can be apprehended directly, without benefit of theory. Reality is whatever we directly observe. In a more moderate version of positivism, theory is but one step removed from reality, is arrived at largely by induction, is rather easy to construct, and is fairly easy to test. In their book on interdependence, Keohane and Nye provide a clear example when they "argue that complex interdependence sometimes comes closer to reality than does realism" (1989, 23). Yet, if we knew what reality is, theory would serve no purpose. Statements such as "parsimony is a judgment . . . about the nature of the world: it is assumed to be simple," neatly express the idea that theory does little more than mirror reality (King, Keohane, and Verba 1994, 20)

Faced with an infinite number of "facts" one must wonder, however, which ones are to be taken as pertinent when trying to explain something. As the molecular biologist Gunther Stent has put it: "Reality is constructed by the mind . . . the recognition of structures is nothing else than the selective destruction of information" (1973, E17). Scientists and philosophers of science refer to facts as being "theory laden" and to theory and fact as being "interdependent." "Every fact," as Goethe nicely put it, "is already a theory." Theory, rather than being a mirror in which reality is reflected, is an instrument to be used in attempting to explain a circumscribed part of a reality of whose true

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dimensions we can never be sure. The instrument is of no use if it does little more than ape the complexity of the world. To say that a "theory should be just as complicated as all our evidence suggests" (King, Keohane, and Verba 1994, 20) amounts to a renunciation of science from Galileo onward.

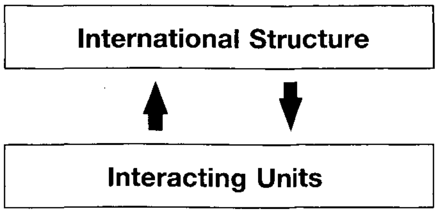
Because of the interdependence of theory and fact, the construction and testing of theories is a more problematic task than most political scientists have thought. Understanding this, Lakatos rejected "dogmatic falsification" in favor of judging theories by the fruitfulness of the research programs they may spawn. Following Lakatos, Vasquez faults the realist paradigm for what he takes to be the regressive quality of its research program. Forsaking Lakatos, he then adduces evidence that in his view falsifies balance-of-power theory in its structural-realist form. I shall consider both claims.

I disagree with Lakatos on some points, but not on his rejection of the notion that tests can falsify theories. To explain why falsification won't do, I all too briefly mention two problems. First, proving something false requires proving something else true. Yet the facts against which we test theories are themselves problematic. As Lakatos rightly says, in italics, "*theories are not only equally unprovable, . . . they are also equally undisprovable*" (1970, 103; cf. Harris 1970, 353). Among natural scientists, falsification is a little used method (Bochenski 1965, 109; cf. Harris 1970). Social scientists should think about why this is so.

Second, citing Popper (1959), Vasquez insists that "paradigms" should specify the evidence that would disprove them and criticizes realism for not doing so (p. 905). In contrast, Lakatos observes that "*the most admired scientific theories simply fail to forbid any observable state of affairs*" (1970, 100, his italics). This is true for many reasons. Lakatos himself points out that we always evaluate theories with a ceteris paribus clause implied, and we can never be sure that it holds. To express the same thought in different words, scientific theories deal in idealizations. If the results of scientific experiments are carried to enough decimal points, hypotheses inferred from theories are always proved wrong. As the Nobel Laureate in physics, Steven Weinberg, puts it: "There is no theory that is not contradicted by some experiment" (1992, 93). Ernst Nagel (1961, 460–6, 505–9) expressed a similar thought when he pointed out that social-science predictions fail because social scientists do not deal in idealizations. It is because falsification is untenable that Lakatos proposes that we evaluate theories by the fruitfulness of their research programs. Ultimately, he concludes, as others had earlier, that a theory is overthrown only by a better theory (p. 119; cf. Conant 1947, 48).

Despite claiming to follow Lakatos's advice to evaluate theories through their research programs, Vasquez emphasizes what he takes to be evidence falsifying balance-of-power theory. According to him, the historian Paul Schroeder (1994) has presented "devastating evidence" against it. One must understand, however, what a theory claims to explain before attempting to test it. Early in his piece, Schroeder (p. 109) draws a picture of neorealism's logic. All of his arrows run in one direction, from the system downward. Realizing that many people have trouble understanding theory, I drew a few pictures myself. Figure 1 depicts one of them (Waltz 1979, 40). Structural theory emphasizes that causation runs from structures to states *and* from states to structure. It also explains, among other things, why balances of power recurrently form. Schroeder rejects structural theory because it fails to account for the motives of statesmen. Yet, as William Graham Sumner wrote: "Motives from which men act have nothing at all to do with the consequences of their action" ([1911] 1968, 212). I would say "little" rather than "nothing," but the point is clear, and structural theory explains why it holds. What Vasquez takes to be Schroeder's "devastating evidence" turns out to be a melange of irrelevant diplomatic lore. Like Vasquez, Schroeder ignores the basic injunction that theories be judged by what they claim to explain. Moreover, both fail to notice that Morgenthau's understanding of balances of power differs fundamentally from mine. For Morgenthau, balances are intended and must be sought by the statesmen who produce them. For me, balances are produced whether or not intended. Schroeder's "evidence" may apply to Morgenthau's ideas about balances of power; it does not apply to mine. This again shows how misleading it is to place all realists in a single paradigm.

FIGURE 1



Vasquez and Schroeder note that power is often out of balance. Is structural theory invalidated because the actions of states sometimes fail to bring their system into balance? In answering this question, it is helpful to think of similar problems in economics. Classical economic theory holds that, in the absence of governmental intervention, competitive economies tend toward equilibrium at full employment of the factors of production. Yet one rarely finds an economy in equilibrium. Further, theory leads one to expect that competition will lead to a similarity of products as well as of prices. Illustrating the result, Harold Hotelling (1929) pointed out that autos, furniture, cider, churches, and political parties become much like one another. But a tendency toward the sameness of products may not be apparent at a given moment, for a competitor may successfully outflank its rivals by offering a design that breaks the mold. Do economies in disequilibrium and variations in product design cast doubt on hypotheses inferred from theories of competition? Hardly. Economic theory predicts strong and persistent tendencies rather than particular states or conditions. Similarly, no contradiction exists between saying that international-political systems tend strongly *toward* balance but are seldom *in* balance.

Vasquez's attempt to apply Lakatos's ideas about research programs to balance-of-power theory is as unsuccessful as his attempt to adduce evidence that would falsify it. Lakatos defines a series of theories as progressive "if each new theory has some excess empirical content over its predecessor, that is, if it predicts some novel, hitherto unexpected facts" (1970, 116). Newtonian science is a wonderful example of a progressive series of theories, incorporating the same basic assumptions about the universe in theories covering successively more phenomena. Classical economics, able to explain the working of national and of international economies as well, is another example. In international politics, where can one find such a use of fundamental concepts to develop theories covering ever more phenomena? Vasquez claims to find several, but his claim rests sometimes on placing in a single realist program work that belongs in different ones, and sometimes on taking work done when applying a theory as being the creation of a new one.

One cannot judge the fertility of a research program by evaluating work done outside of it. Vasquez takes Randall Schweller's (1994) essay on bandwagoning as work done within the realist paradigm and argues that it provides an example of its degeneration. Schweller, however, sets out to show that the central theory of neorealism is wrong. He rejects neorealism's assumptions about power as a means and survival as the goal of states in favor of Morgenthau's assumption that states seek ever more power. He claims to show that bandwagoning is more common than balancing, believing that if it is, then neorealist theory fails. Schweller and I work within different research programs. The question therefore shifts from the quality of the program to whether his claims about bandwagoning invalidate structural theory.

Structural theory assumes that the dominant goal of states is security, since to pursue whatever other goals they may have, they first must survive. Bandwagoning and balancing by the logic of the theory are opposite responses of security-seeking states to their situations. States concerned for their security value relative gains over absolute ones. At the extremes, however, with very secure or very insecure states, the quest for absolute gains may prevail over the quest for relative ones. Very weak states cannot make themselves secure by their own efforts. Whatever the risks, their main chance may be to jump on a bandwagon pulled by stronger states. Other states may have a choice between joining a stronger state and balancing against it, and they may make the wrong one. States sometimes blunder when trying to respond sensibly to both internal and external pressures. Morgenthau once compared a statesman not believing in the balance of power to a scientist not believing in the law of gravity. Laws can be broken, but breaking them risks punishment. One who violates the law of gravity by stepping from a nineteenth-story window will suffer instant and condign punishment. A state that bandwagons when the situation calls for balancing runs risks, as Mussolini's Italy discovered after it jumped on Hitler's bandwagon, although in international politics punishment may not be swift and sure. By joining the stronger side, Italy became Germany's junior partner, and Mussolini lost control of his policy. Bandwagoning by some states strengthened Germany and encouraged Hitler to further conquest. Only balancing in the middle and later 1930s could have stopped him. Various states, including Italy, paid a great price for their failure to balance earlier. Theory does not direct the policies of states; it does describe their expected consequences.

States' actions are not determined by structure. Rather, as I have said before, structures shape and shove; they encourage states to do some things and to refrain from doing others. Because states coexist in a self-help system, they are free to do any fool thing they care to, but they are likely to be rewarded for behavior that is responsive to structural pressures and punished for behavior that is not.

Vasquez requires that theories predict, since prediction seems to make falsification possible. He therefore seizes upon Schweller's claim that bandwagoning is more common than balancing. Whether this looks like falsifying evidence depends on what is predicted. Like classical economic theory, balance-of-power theory does not say that a system will be in equilibrium most or even much of the time. Instead, it predicts that, willy nilly, balances will form over time. That, Vasquez would no doubt say, is not much of a prediction. Yet Charles Kegley (1993, 139) has sensibly remarked that if a multipolar system emerges from the present unipolar one, realism will be vindicated. Seldom in international politics do signs of vindication appear so quickly. Multipolarity is developing before our eyes: To all but the myopic, it can already be seen on the horizon. Moreover, it is emerging in accordance with the balancing imperative.

In the light of structural theory, unipolarity appears as the least stable of international configurations. Unlikely though it is, a dominant power may behave with moderation, restraint, and forbearance. Even if it does, however, weaker states will worry about its future behavior. America's founding fathers warned against the perils of power in the absence of checks and balances. Is unbalanced power less of a danger in international than in national politics? Some countries will not want to bet that it is. As nature abhors a vacuum, so international politics abhors unbalanced power. Faced by unbalanced power, states try to increase their own strength or they ally with others to bring the international distribution of power into balance. The reactions of other states to the drive for dominance of Charles I of Spain, of Louis XIV and Napoleon Bonaparte of France, of Wilhelm II and Adolph Hitler of Germany, illustrate the point.

Will the preponderant power of the United States elicit similar reactions? Unbalanced power, whoever wields it, is a potential danger to others. The powerful state may, and the United States does, think of itself as acting for the sake of peace, justice, and well-being in the world. These terms, however, will be defined to the liking of the powerful, which may conflict with the preferences and interests of others. The powerful state will at times act in ways that appear arbitrary and high handed to others, who will smart under the unfair treatment they believe they are receiving. Some of the weaker states in the system will therefore act to restore a balance and thus move the system back to bi- or multipolarity. China and Japan are doing so now.

In international politics, overwhelming power repels and leads others to balance against it. Stephen Walt (1987, viii, 5, 21, 263–5) has offered a reformulation of balance-of-power theory, believing that states balance not against power but against threat. Vasquez sees Walt's "refinement" as placing a semantic patch on the original theory in an attempt to rescue it from falsifying evidence. I would agree if I took Walt's reformulation to be the correction of a concept that increases the explanatory power of a defective theory and makes it more precise. Changing the concepts of a theory, however, makes an old theory into a new one that has to be evaluated in its own right. I see "balance of threat" not as the name of a new theory but as part of a description of how makers of foreign policy think when making alliance decisions. Theory is an instrument. The empirical material on which it is to be used is not found in the instrument; it has to be adduced by the person using it. Walt makes this clear when he describes "threat" as one of the "factors that statesmen consider when deciding with whom to ally" (p. 21). In moving from international-political *theory* to foreign-policy *application* one has to consider such matters as statesmen's assessments of threats, but they do not thereby become part of the theory. Forcing more empirical content into a theory would truly amount to a "regressive theory shift." It would turn a general theory into a particular explanation. Vasquez, and Walt, have unfortunately taken the imaginative application of a theory to be the creation of a new one.

Vasquez makes a similar mistake in his appraisal of Christensen's and Snyder's (1990) essay, "Chain Gangs and Passed Bucks." "The authors," according to Vasquez, "find a gap in Waltz's explanation [of European diplomacy preceding World War II] and try to correct it by bringing in a variable from Jervis" (p. 906). However good or bad my brief explanation of what happened in Europe prior to World War II may be (Waltz 1979, 164–70), an explanation is not a theory. A theory does not provide an account of what has happened or of what may happen. Just as a hammer becomes a useful tool when nails and wood are available, so a theory becomes useful in devising an explanation of events when combined with information about them.

The question is not what should be included in an account of foreign policies but what can be included in a theory of international politics. A theory is not a mere collection of variables. If a "gap" is found in a theory, it cannot be plugged by adding a "variable" to it. To add to a theory something that one believes has been omitted requires showing how it can take its place as one element of a coherent and effective theory. If that were easy to do, we would be blessed with a wealth of strong and comprehensive theories.

I conclude by emphasizing a few points about the testing of theories. A theory's ability to explain is more important than its ability to predict. At least Steven Weinberg and many others think so. Believing that scientists will one day come up with a final theory, he writes that even then we will not be able "to predict everything or even very much," but, he adds, we will be able to understand why things "work the way they do" (1992, 45; cf. Toulmin 1961, 36–8). Success in explaining, not in predicting, is the ultimate criterion of good theory. Theories of evolution, after all, predict nothing in particular.

Vasquez makes the testing of theories seem easy by adopting a positivist standard: Does the observation made correspond with a theory's prediction? His adoption of such a standard is shown by his crisp assertion that the failure of states to balance "in the period before World War II . . . should be taken as falsifying evidence" (p. 906). Yet what is to be taken as evidence for or against a theory is always in question. Some attempts to balance were made in the prewar years, but a balance formed, so to speak, only in the end. Should delay in completing a balance be taken as evidence contradicting balance-of-power theory? One may not be able to answer the question decisively. Testing theories is an uncertain business. In this case, however, one should certainly remember that the theory being tested explains the process of balancing as well as predicting that balances recurrently form. The theory cannot say how long the process will take.

The title of Errol Harris's (1970) book, *Hypothesis and Perception*, implies a criticism of Popper's claim that a critical test of a hypothesis, if flunked, falsifies a theory once and for all. As Harris suggests, our perceptions count; the results of tests require interpretation. Evaluating a theory requires working back and forth between the implications of the theory and an uncertain state of affairs that we take to be the reality against which the theory is tested. Whether or not events in the 1930s tend to validate or to falsify my version of balance-of-power theory depends as much on how one interprets the theory as on what happened. However thorough the evaluation of a theory, we can never say for sure that the theory is true. All the more, then, we should test a theory in all of the ways we can think of—by trying to falsify and to confirm it, by seeing whether things work in the way the theory suggests, and by comparing events in arenas of similar structure to see if they follow similar patterns. Weinberg suggests yet another way. "The most important thing for the progress of physics," he writes, "is not the decision that a theory is true, but the decision that it is worth taking seriously" (1992, 103). The structural theory set forth in my *Theory of International Politics* at least passes that test.

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Progressive Research on Degenerate Alliances

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Our article (1990) on alliance dynamics in multipolarity, which John Vasquez criticizes, differs from some neorealist approaches by incorporating variables from the theory of the security dilemma, including perceptual factors. Nonetheless, our argument meets Imre Lakatos's criteria for a progressive problem shift insofar as it explains more than the original theory, does so parsimoniously, has been successfully applied to new domains, and does not introduce new assumptions that contradict the core of the original theory.

John Vasquez is right to insist that students of international politics should justify their theories in terms of Imre Lakatos's (1970) criteria for distinguishing progressive research programs from degenerative ones. Indeed, in our article on European alliance patterns (Christensen and Snyder 1990) we paid close attention to these criteria. We are grateful for the opportunity to remind readers of our strategies for theory-building and of the empirical and theoretical puzzles that motivated us to write the original article.

Before responding to Vasquez's (1997) specific criticisms of our arguments, we must clarify the general relationship between our article and the enterprise of neorealism. We are not hard-core neorealist disciples whose main motivation is to "explain away" anomalies in the neorealist research program, as Vasquez contends. In common with neorealists, we believe that considerations of power often play a central role in the calculations of state leaders and that the logic of anarchy often shapes their strategic choices. We also believe, however, that domestic politics, perceptions, ideology, and other factors may play a major role in shaping choices and outcomes. Sometimes these factors may override the considerations that neorealists emphasize. More commonly, we believe that such factors interact with or mediate the effect of neorealist factors, such as the international distribution of power and the degree of military vulnerability. We adopt this perspective in our article on alliances and in many of our other works (for example, Christensen 1996, Snyder 1991). Whether this makes us neorealists is a labeling exercise in which we decline to take part.

We resist being cast in the role of apologists for neorealism. Still less are we interested in trying to reconcile the heterogeneous arguments of the various scholars whom Vasquez calls neorealist. Nonetheless, we do take the view that Waltz and other scholars working in a similar mode have generated insights into international politics that can be employed as tools in our broader, more eclectic research programs. Consequently, we felt we had a stake in sharpening those tools in order to use them more productively to solve a specific problem that concerned us: namely, the conditions that lead to unconditional "chain-gang" alliances as a response to threat in multipolarity, as opposed to "passing the buck" to third parties to parry the threat. We think that our borrowings from neorealism for that specific purpose yielded valid insights, when employed in the context of our overall research design. Moreover, our argument constitutes a progressive problem shift, as judged by Lakatos's criteria.

STRATEGIC PERCEPTIONS AND ALLIANCE CHOICES

Before discussing how our article fulfills those criteria, we will briefly restate our central arguments; except for one footnote (11), Vasquez (1997) misses our main point entirely. We began by noting that alliance patterns before the two world wars were very different, in fact opposite, yet the polarity of the international system, as defined by Waltz, was very similar. For the most part, the European powers chain-ganged as a response to the German threat before 1914 and buck-passed in the face of a similar threat in the 1930s, despite the similar multipolar configurations of power. Therefore, Waltz's ultraparsimonious structural analysis, focused exclusively on polarity, cannot explain the variation.

Of course, Waltz (1979, 1996) explicitly and repeatedly states in his work that, in proposing a structural realist theory, he is not interested in explaining foreign policy. "Waltz is interested in showing that a system of two is more stable than a system of many. He therefore evinces no interest in predicting which pathology of multipolarity will appear in particular circumstances" (Christensen and Snyder 1990, 142). Thus, the problem that interested us could be viewed as largely irrelevant to Waltz's concerns. Notwithstanding these differences in objective, we found Waltz's analysis of the dynamics of multipolar and bipolar worlds a useful starting point. This, however, no more makes us slavish neorealists than emphasizing the economic sources of political power makes one an orthodox Marxist. Although multipolarity was a factor in both the quick escalation of the July crisis in 1914 and the lethargic response to Hitler in the late 1930s, identifying which multipolar malady occurs in which circumstances requires looking for additional causal factors.

To unravel this puzzle, we argued that, if leaders in a multipolar setting believe that offensive doctrines are likely to be effective, then they are more likely to form tight alliances; if they believe defenses are hardy, then they are more likely to attempt to pass onto their current or potential future allies the high costs of initial resistance to aggressors. Under multipolar conditions, when leaders believe wars will be decided early, chain-gangs are likely, and when they expect war to be attritional, buck-passing is likely. Strategic realities are filtered through leaders' perceptions, however, and leaders often misperceive the objective strategic environment, as they did before both world wars. So, in our approach, leaders' perceptions of the strategic environment, not objective conditions, constitute the central variable that determines outcomes.

Except for one footnote, Vasquez ignores this perceptual aspect of our argument, misreading it as a purely structural account intended to save Waltzian neorealism's arguments about states' tendency to balance against threats. This misreading is also the source of Vasquez's claim that our argument constitutes a degenerative problem shift for neorealism, on the grounds that it "hides" or "explains away" what he sees as the failure of interwar states to balance. Far from hiding lacunae in the realist account about multipolar instability, we ourselves uncovered and explained them using perceptual variables that orthodox neorealists typically underrate.

Vasquez also fails to recognize that our dependent variable is not "balancing" but alliance maladies that complicate the balancing process. Failures to balance properly occurred before both wars, not just World War II. We wrote: "In multipolarity, two equal and opposite alliance dilemmas impede efficient balancing" (Christensen and Snyder 1990, 140). Before World War I, states were hyperactive; before World War II, they were lethargic. Thus, we are not testing some generic proposition about "the law of balancing." Rather, we are exploring implications of Waltz's more specific arguments about the mechanisms that make multipolar worlds less stable than bipolar ones. This means that, all things being equal, balancing occurs less smoothly in multipolar worlds than in bipolar ones. Vasquez's claim that Waltz should expect multipolar alliances to form smoothly runs counter to one of the major themes, if not the major theme, of Waltz's work. Thus, Vasquez's arguments about alliance dynamics before the world wars are unhelpful as criticisms either of our work or that of Waltz.

MEETING LAKATOS'S STANDARDS

Having clarified the objectives of our article, we can now assess whether it satisfies Lakatos's criteria for a progressive problem shift. One criterion is whether the new theoretical formulation can explain phenomena that the original one cannot. We pass that test, since we explain when chain-ganging occurs and when buck-passing occurs, whereas Waltz only explained that both were more common in multipolarity than in bipolarity. A second criterion is whether the new formulation can explain a large number of new observations, or whether each additional observation demands ad hoc adjustments to the theory. As we argue in the historical section below, we pass that test, too, insofar as our formulation adequately explains the alliance choices of all the major European powers before both world wars. A third criterion is whether the new formulation can be generalized successfully to new domains, and whether any theoretical adjustments needed to extend its range can be accomplished parsimoniously. We pass that test, too, given that a simple extension of our original argument allowed Christensen (1997), in a follow-up article, to explain a variety of nineteenth-century alliance choices. Unlike degenerative Ptolemaic astronomy, we explain a lot with a little.

A final criterion is whether modifications to the original theory introduce assumptions that contradict the theory's core logic. Our article took great pains to show that Waltz's basic assumptions overlap extensively with the core axioms of Robert Jervis's (1978) theory of the security dilemma. This theory has always been understood as having dove-tailed structural and perceptual elements. In anarchy, actors may misperceive structural constraints, but insofar as their perceptual antennae remain focused on figuring out what those structural constraints are, we contend that perceptual arguments are neither contradictory to structural ones nor especially degenerative. Moreover, as Waltz argues, alliance dynamics are more complex and uncertain in multipolarity. Therefore, under these structural conditions, miscalculation and misperception should be more likely.

VASQUEZ'S HISTORICAL ANALYSIS

Aside from his theoretical arguments, Vasquez calls into question some of our historical interpretations. He criticizes our account of alliance behavior before the two world wars on two counts. Before World War I, he argues, Britain did not behave in a way consistent with our theory. It did not chain itself tightly to allies and, according to Vasquez, did less than it did in the early phases of World War II. He also suggests that in the 1930s there was really no balancing against the rise of German power. Instead there was appeasement, something he believes is fundamentally at odds with balancing.

Once again, Vasquez misrepresents our article. We stated clearly that Britain did not chain-gang in 1914 to the same degree that France and Russia did, in part because Britain's off-shore strategic position gave it a defensive advantage that the continental states lacked. Thus, British behavior differed for reasons that our hypothesis readily explains (Christensen and Snyder 1990, 155–6).

Consistent with our argument, Britain responded rather slowly to the security challenges on the continent before both world wars. It is inaccurate to state that it was slower to respond in 1914 than in 1939. British faith in defensive advantage on land was much stronger in 1939 than in 1914, and Britain's commitment to Europe was therefore much smaller and slower to arrive than its commitment to France in summer 1914. London managed to assemble no more than ten divisions for the British Expeditionary Force by the time Germany invaded the Low Countries and France in May 1940 (eight months after the Anglo-French declaration of war). That force was only about half the size sent to Europe by the first half of 1915, and it constituted only 10% of joint Anglo-French forces on the continent (Christensen 1997). Finally, regarding Vasquez's comparison between August 1914 and March 1939, our theory readily explains why the Serbian question sparked quick escalation in 1914, whereas the strategically much more important issue of German control of Czechoslovakia did not do the same in 1938–39.

On the issue of "appeasement versus balancing" Vasquez asks (p. 907): "Were Britain, France, and the USSR passing the buck in the late 1930s, or were they just slow to balance? Or were Britain and France pursuing an entirely different strategy, appeasement?" In fact, Britain, France, and Russia were slow to balance and also were buck-passing. The reasons for both postures were similar: British and Russian leaders in particular underestimated the threats posed by German offensives against France and the Low Countries. Therefore, in both their internal balancing efforts (arming) and their external balancing efforts (alliances) they responded anemically in the short term in order to preserve fighting power for the longer war that they anticipated would follow Germany's initial attempts to subdue either Britain or France.

British, French, and Russian leaders were still concerned about the growth of German power and aggressiveness, however. It is wrong to suggest that they were "appeasing" instead of "balancing." Once again, they did both. It is strange that Vasquez takes an "either/or" view of realist balancing and diplomatic appeasement. One of the first major realist works (published 40 years before Waltz's *Theory of International Politics*) was E. H. Carr's classic *Twenty Years' Crisis*. The 1939 edition of this book called on England to appease Hitler in the short term and to build up military power to balance against German expansionism in the longer term (Carr 1939). Appeasement is a diplomatic strategy that can either accompany or preclude balancing strategies, in the same way that "talking tough" and leveling coercive threats can accompany or preclude taking concrete measures to improve one's power position in the world.

Moreover, Britain's misguided and dangerous appeasement policy was largely abandoned after March 1939. By then, British strategists were thinking almost solely in terms of balancing against German power. Britain and France both increased their defense spending in 1939–40. Moreover, they formed an alliance in early 1939. Clearly, some balancing was occurring. Yet, Britain continued to adopt rather anemic responses to the threat posed to France and the Low Countries, even after the formation of a formal Anglo-French alliance. The pace and direction of the British rearmament effort as well as the deployment of available forces all demonstrated Britain's false faith in the hardiness of defensive positions in northern and eastern France (Christensen 1997).

As for the Soviet Union, Stalin's appeasement of and collusion with Hitler were accompanied by a serious arms build-up. As we reported in the original article, Stalin was shocked that France fell so quickly in spring 1940 (Christensen and Snyder 1990, 157). He not only underestimated the effectiveness of German offensives but also grossly overestimated the comprehensive power of Britain and France in comparison to Germany. Given his confidence about French defenses and the power of the Anglo-French condominium, Stalin was much less interested in alliance formation in the months leading up to Guderian's blitz than he likely otherwise would have been (Christensen 1997).

Stalin's misjudgements and the ways that they affected Soviet strategy demonstrate particularly well the real shortcomings in straightforward realist foreign policy analysis. Even when state leaders think of their security in terms consistent with realist tenets—weighing the general balance of power and the ways in which military power can be used to threaten national security—leaders still will often analyze their security environment so badly that they will adopt policies that should appear puzzling to realists.

By underreacting to the threat posed by Hitler, the British, French, and Soviet leaders did not maximize the security of the future alliance. But to suggest that they did not balance at all is simply wrong. The most obvious fact in this regard is that Germany was defeated. If no one balanced, then how did this happen? Soviet, French, and British leaders built up their militaries beginning in 1938, and Britain and France formed an alliance several months before the German invasion of Poland. Britain sent ten divisions to its ally before the German invasion of France; British leaders considered sending more immediately thereafter, until they received the shocking news of the total collapse of allied forces; finally, after the fall of Paris, London guaranteed support for the remaining French forces, thus assuring that Britain would be Hitler's next target. Though Britain should have done more to help France and should have done so earlier, it hardly "distanced" itself from the crisis in France (Christensen 1997).

When designing their security policies before the Cold War, European leaders did think in terms of the balance of power (both the number of great powers and the distribution of power among them) and the relative efficacy of offensive versus defensive strategies. Although they worried about their national security in ways familiar to realists, they often misread their strategic environments on both counts, and thus behaved in ways that should confound realists. This is true not only for the early twentieth century but also for the late nineteenth century (Christensen 1997).

This does not mean that all regions in all times have leaders who are obsessed first and foremost with Realpolitik. Rather than offering a theory of all foreign policy, we were offering a perceptual approach to explain why behaviors vary greatly even in structurally similar worlds in which leaders are generally thinking in terms of power and military doctrine. So, in this limited sense, our article may be seen as truly critical of realism. It uncovers the problems of straightforward realism in explaining the cases in which realism should prove most effective, and it explains the anomalies using perceptual variables. But we never meant to suggest that even our "emendation" of realism would prove decisive in analyzing all international settings.

Just as it would be very dangerous to try to ignore power-related factors such as multipolarity, leaders' calculations of the balance of power, and beliefs about the offense-defense balance in analyzing security relations in pre–Cold War Europe, it would be equally dangerous to assume that power politics is always primary in the thinking of all leaders in relations with all outsiders. Both dogmatic realism and dogmatic antirealism will prove crippling in understanding many aspects of international relations. Ideology, institutions, domestic politics, and interdependence may indeed combine in various ways that make balance-of-power considerations irrelevant to various regions or bilateral relationships (Goldgeier and McFaul 1992, Jervis 1991/92). Yet, there have been, there still are, and there likely will be again times and places in which Realpolitik thinking prevails. Our 1990 article and Christensen's 1997 article merely emphasize that, to understand such worlds, we will have to know more about the region in question than the balance of power or the relative efficacy of offensive and defensive doctrines. We will have to know what leaders think about those factors in order to explain and predict the likely patterns of alliance formation and potential causes of conflict escalation.

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Lakatos and Neorealism: A Reply to Vasquez

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We disagree that our correspondence (Elman and Elman 1995) regarding Schroeder (1994) supports Vasquez's (1997) verdict that the neorealist scientific research program is degenerating. We argue that Vasquez's conclusion is based on a misstatement of the Lakatosian criteria of appraisal and a mistaken conflation of the neorealist research program with the proposition that balancing is a common foreign policy. We do, however, welcome Vasquez's attempt to apply Lakatosian metatheory to international relations theory, and we hope that this conversation will encourage others to follow his lead.

John Vasquez (1997) provides an account of Lakatos's (1970) methodology of scientific research programs and then uses the criteria to conclude that the neorealist scientific research program is degenerating. In support of his claim, Vasquez references our commentary (Elman and Elman 1995) on Schroeder's (1994) thoughtful and important critique of neorealism. In particular, Vasquez insists that two of our arguments are signs of degeneration: (1) Evidence that states do not balance may challenge Waltz's (1979) theory without necessarily undermining the neorealist research program; and (2) a program should be improved before it is discarded and, in any event, should not be rejected until an alternative is devised to take its place. We stand by both arguments and maintain that they do not constitute evidence of neorealism's degeneration. We suggest that Vasquez's conclusion is based on a misstatement of Lakatos's criteria and on a mistaken conflation of the neorealist program with the proposition that states balance.

Vasquez presents four indicators of a degenerating research program and argues that "Elman and Elman (1995) serves as evidence that all these are very much in play within the field" (p. 909). His critique, however, is based on a problematic account and application of Lakatos's criteria of appraisal. Vasquez suggests that "it would be highly desirable to construct operational indicators of the progressive or degenerating nature of a . . . research program. Since these are not available, [I] will explicitly identify the characteristics that will be used to indicate that a research program is degenerating" (p. 900). In fact, there is a voluminous literature on Lakatos's methodology, including extended discussions of the operational criteria for appraising scientific research programs. These criteria address two sorts of scientific development: *intra-* or within program problemshifts that consist of modifications to the protective belt of auxiliary hypotheses, and *inter-* or between program problemshifts that occur when, despite the prohibition against modification, elements of the hard core of the research program are rewritten. Problemshifts are degenerative to the extent that they are ad hoc. Lakatosians distinguish among three notions of ad hocness: ad hoc_1, the theory predicts no novel consequences; ad hoc_2, the theory's novel predictions have not been corroborated; and ad hoc_3, the theory is obtained from its predecessor through a modification of the auxiliary hypotheses that does not accord with the positive heuristic of the program (Lakatos 1970, 175 n2 and n3, 182; Musgrave 1974, 20; Zahar 1973, 101). While interprogram problemshifts need only satisfy the two novel fact criteria, intraprogram problemshifts must also correspond with the research program's positive heuristic (Lakatos 1970, 175–6). In light of these widely recognized Lakatosian measures, Vasquez's four "operational criteria" of degeneration (pp. 900–2) prove to be either otiose or incomplete.

Vasquez's first sign of degeneration, that problemshifts are driven by a need for repair in the face of existing empirical anomalies (p. 901), is superfluous. Anomaly solving is neither a necessary nor sufficient indication of degeneration. It is not necessary because anomaly solving is only one motive that may produce problemshifts that do not satisfy the need for novelty (Carrier 1988, 206). It is not a sufficient sign of degeneration because solving an anomaly can be an entirely legitimate exercise, so long as the problemshift also satisfies the three Lakatosian criteria.

The second operational indicator mentioned by Vasquez roughly corresponds to the ad hoc_1 criterion of the prediction of novel facts, but he fails to note the extensive debate among Lakatosian scholars as to what counts as a novel fact (Carrier 1988, 206; Hands 1991a, 70, 1991b, 94). There are at least seven definitions, all following what Musgrave (1974, 3) describes as a "historical approach to confirmation." This is not merely a matter of semantics, because the way factual novelty is defined crucially affects whether a problemshift will be appraised as progressive or degenerating. Since employing different definitions of novel fact will produce contrasting conclusions concerning the progressiveness or degeneracy of the research program under scrutiny, it is quite impossible to apply Lakatos's methodology without first recognizing, and then making a choice from among, the menu of available meanings.[1] This Vasquez fails to do. Indeed, he seems

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[1] The different definitions of novelty can be grouped into four categories: (1) Lakatos_1, or strict temporal novelty (Hands 1991b, 96; Lakatos 1970, 118; Nunan 1984, 275; Worral 1978, 46 and 66, n7; Zahar 1973, 101); (2) Lakatos_2, or new interpretation novelty

to rely on several different definitions throughout his essay.

Vasquez's third operational indicator is an incomplete version of the ad hoc_2 criterion that the new theory's novel predictions are not borne out by empirical evidence (Zahar 1973, 101, n1). Vasquez fails to tackle the thorny issue of dating the novel predictions made by the research program (Rosenberg 1986, 135). One can make a case for a variety of different starting dates for the neorealist research program and variants created by problemshifts both within and from it. Vasquez also declines to choose from between the two available versions of ad hoc_2: that none of the excess content is corroborated or that all its excess content is refuted (Zahar 1973, 101, n1). Nor does Vasquez specify the time we should allow for either of these two conditions to be satisfied before concluding that a problemshift is ad hoc_2. We do, however, get the impression that Vasquez ignores Lakatos's (1970, 116, 133–4, 155) injunction that scholars should not require instant gratification or be too impatient.

Vasquez's fourth indicator suggests that if "collectively the family of theories fields a set of contradictory hypotheses which greatly increase the probability of at least one passing an empirical test, then a research program can be appraised as degenerating" (p. 901). This indicator is misleading, since it simply describes the ordinary development of a research program. According to Lakatos, changes in the protective belt of auxiliary hypotheses are *designed* to protect the hard core. These intraprogram problemshifts combine the hard core with different auxiliary hypotheses to produce dissimilar predictions. That is, contrary to Vasquez's claim, Lakatos *anticipates* that a family of theories based on the same inviolable core will make contradictory predictions. Thus, for example, the fact that Gilpin (1981) and Waltz (1979) "predict contradictory things" (pp. 908–9) is not an indicator of the degenerating nature of the neorealist research program.

Finally, Vasquez does not mention the ad hoc_3 criterion, modification of the protective belt of auxiliary hypotheses in ways that do not accord with the positive heuristic of the research program. Lakatos (1970, 175) argues that "in the positive heuristic of a powerful programme there is, right at the start, a general outline of how to build the protective belts." He defines "a research programme as degenerating even if it anticipates novel facts but does so in a patched-up development rather than by a coherent, pre-planned positive heuristic" (Lakatos 1971b, 125). Vasquez does not specify the neorealist positive heuristic, nor does he address this form of ad hocness in his overview of contemporary international relations theory.

Accordingly, although Vasquez argues that our 1995 essay provides evidence of neorealism's Lakatosian degeneration, his critique is based on incomplete and inconsistent operational measures. We suggest, however, that even when judged by the true Lakatosian criteria, our 1995 arguments fail to support his conclusion that the neorealist program is degenerating. Vasquez first cites our point that evidence that states do not balance may challenge Waltz's theory without directly undermining the neorealist research program. Vasquez charges that permitting both balancing and nonbalancing behavior to be consistent with neorealism is evidence of the research program's degenerating character (p. 909). This assertion is based on a misspecification of the neorealist program.

Recall that Lakatos argues that research programs have four elements: a hard core consisting of unchanging, privileged knowledge; a negative heuristic which forbids that knowledge from being directly challenged; a protective belt of auxiliary hypotheses, which "bear the brunt of tests and get adjusted and re-adjusted, or even completely replaced, to defend . . . the core" (Lakatos 1970, 133); and a positive heuristic that "guides the production of specific theories within the programme" (Worral 1978, 59; see also Lakatos 1970, 135). An application of the methodology of scientific research programs requires that all these components be specified (de Marchi 1991, 17). Vasquez fails to provide such a description. Instead, he observes that Walt, Schweller, and Christensen and Snyder are working within the same research program because they "all share certain concepts, are concerned with balancing, and share a view of the world and the general purpose of trying to work within and defend the paradigm" (p. 904). This rendition of the neorealist program is methodologically and substantively unsatisfactory. It makes no attempt to delineate neorealism's hard core and heuristics, and it mistakenly conflates the entire neorealist program with balancing.

While we do not have the space to elaborate, we can provide a preliminary and illustrative description of the neorealist hard core.[2] (1) States interact in an anarchic environment, without the protection offered by an overarching authority. (2) States are self-regarding, and consequently self-help is the system-mandated behavioral rule. (3) Threat to survival is the main problem generated by the system, and consequently state behavior is designed to ensure survival. (4) States are the primary actors in international politics. (5) States have limited resources that they can employ to improve their chances for survival, and accordingly they are concerned to maintain or enhance those capabilities. (6) States select those strategies in which the expected gain is likely to exceed the expected loss. (7) States weigh options and make decisions based on their strategic situation and an assessment of the external environment.

(Carrier 1988, 206; Lakatos 1970, 157; Musgrave 1974, 11; Nunan 1984, 275; Zahar 1973, 102); (3) $Zahar/Lakatos_3$, or heuristic novelty (Lakatos and Zahar 1975, 375–6; Worral 1978, 48–9; Zahar 1973, 103); and (4) Musgrave's background theory novelty (Carrier 1988, 213; Musgrave 1974, 15–6). Although lack of space prevents an extensive discussion of this debate, we favor Worral's (1978, 48–9) reformulation of $Zahar/Lakatos_3$ novelty: "One can't use the same fact twice: once in the construction of a theory and then again in its support."

[2] This list is derived from, and supporting citations may be found in, Elman and Elman 1995 and Elman 1996.

Our main criticism of Schroeder's (1994) article was that his reading of European history is largely consistent with this neorealist hard core. Schroeder may be right that the historical record of European international relations does not conform to Waltz's (1979) prediction of balancing, but contrary to Schroeder and Vasquez's assertions, neorealist theories do not invariably predict that states will balance; that they will always balance effectively (they are less likely to under multipolarity when structural pathologies are present); or that balances will always form. Specific behavioral predictions depend upon a combination of the hard core with different versions of the protective belt of auxiliary hypotheses. Although we cannot provide a full account of these different versions here, one brief example may be helpful. It is common practice to distinguish between offensive neorealists, who assert that states are power maximizing revisionists, and defensive neorealists, who contend that states defend the status quo and minimize relative power losses (Frankel 1996, xv–xviii; Mearsheimer 1994/95, 11–2, n27). These two broad strategic assumptions funnel neorealist theories in particular directions. It is the particular costs and benefits suggested by these and other elements of the protective belt, in combination with the unchanging hard core, that lead neorealists to predict different behaviors. Employing some versions of the protective belt, strategies other than balancing will have lower costs and a better chance of coping with the external threat. Hence, as we argue in our rejoinder to Schroeder, neorealist theories can predict policies other than balancing, including bandwagoning and integrating (Elman and Elman 1995; see also Elman 1996).

But this is not to argue that virtually any behavior is compatible with the neorealist research program. Vasquez misrepresents our position (p. 909) when he claims we assert that *only* "other-regarding" behavior is inconsistent with the hard core of neorealism (see, for example, Elman and Elman 1995, n6). Neorealism will never, indeed, by definition *cannot*, predict behavior that is incompatible with any elements of the hard core. Hence, it cannot predict that states will behave irrationally, or will respond primarily to demands internal to the state, or behave as if they had other-help identities, and so on.[3] Thus, contrary to Vasquez (pp. 909–10) and Schroeder (1995, 194), neorealism does not "appropriate every possible tenable position in IR theory and history." The nonneorealist box is not empty.

Vasquez also notes as a sign of neorealism's degeneration our argument that, consistent with Lakatos (1970, 115), if a research program is found wanting, it should be improved before it is discarded; in any event it should not be thrown away until an alternative is devised to take its place. Again, as with his fourth operational indicator, Vasquez mistakenly presents what Lakatosians see as the typical trajectory of scientific development as evidence of degeneration. The tactics we suggest in our 1995 essay require amending the protective belt of auxiliary hypotheses in accordance with the research program's positive heuristic. While these problemshifts may turn out to be degenerative when judged by the three Lakatosian criteria of ad hoc$_1$, ad hoc$_2$, and ad hoc$_3$, our suggestion that scholars should amend the protective belt rather than discard the program is entirely consistent with standard Lakatosian metatheory.[4] In and of itself, it certainly does not constitute evidence of degeneration. Moreover, acknowledging that only a better theory can replace a theory recognizes that the methodology of scientific research programs is a comparative metric. A complete judgment of neorealism's progress cannot be made without reference to its rivals (e.g., classical realism, liberal institutionalism) and the extent to which these have been progressive or degenerating (Blaug 1976, 155, 177; Carrier 1988, 276–8; Lakatos 1971a, 177, 1971b, 105; Nunan 1984, 271–3, 281–2, 290).

Although we disagree with Vasquez's critique of our 1995 essay, and with his conclusions regarding the neorealist program, we welcome his attempt to apply the methodology of scientific research programs to international relations theory. Scholars in the field often claim to be following Lakatos's philosophy of science, but usually this amounts to little more than a boilerplate footnote reference to Lakatos's (1970) seminal essay. Only rarely do we find substantive discussions and applications of the methodology.[5] We applaud Vasquez's effort, and we hope that this exchange of views will induce other scholars to employ this useful and powerful approach.

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[3] There are, of course, nonneorealist theories suggesting otherwise. Hence, some argue that foreign policy behavior can be explained as the product of nonrational choice mechanisms (Katzenstein 1996); internal demands (Milner 1988); or other-regarding identities and corollary behavioral rules (Wendt 1992).

[4] Some Lakatosians would strongly disagree with our suggestion that the methodology of scientific research programs can be used to advise scholars on how to respond to degeneration rather than as a retrospective tool of appraisal (Musgrave 1976, 474). Lakatos (1971a, 174, 178, 1971b, 125) explicitly argues that he offers no advice to the working scientist. He only insists that practitioners should be aware of their alternatives, publicize the decisions they are making, and accept the consequences of their choices (Lakatos 1971b, 104–5). We agree with Blaug (1992, 37) that this position amounts to intellectual schizophrenia, and with Musgrave (1976, 473–82) that Lakatos's attempts to deny the methodology's role in providing advice for working scientists are inconsistent, unwise, and unnecessary.

[5] For scarce examples, see Bueno de Mesquita 1984; Bueno de Mesquita, Krasner, and Jervis 1985; Grieco 1995; Keohane 1986; and Simowitz and Price 1990. By contrast, economics has produced extensive discussions and applications (de Marchi 1991, 29–30).

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New Realist Research on Alliances: Refining, Not Refuting, Waltz's Balancing Proposition

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Realism is both a scientific research program and, more traditionally, a political philosophy. All realists share a pessimistic worldview that posits perpetual struggle among groups for security, prestige, and power and that denies the capacity of human reason to create a world of peace and harmony. Recent research by so-called neotraditional realists does not disconfirm Waltz's balancing proposition. Instead, these works have tended to add unit-level variables in order to transform Waltz's theory of international politics into one of foreign policy. The question is not whether states balance or bandwagon—history clearly shows that they do both—but rather under what conditions states choose one strategy or the other.

Vasquez's (1997) essay is a useful contribution to the enduring controversy over the merits of the realist approach to international relations theory. Disclaimers aside, Vasquez suggests that there is (or should be) one "true theory" of realism and that Waltz's (1979) balancing proposition provides a litmus test for deciding whether realism is a degenerating research program. My response begins with an overarching critique of the piece, in which I challenge both of these claims. This is followed by specific points on Vasquez's treatment of my own work. I conclude by suggesting that realism suffers not from proliferating emendations but rather from underspecified scope conditions.

There is no definitive or "single" theory of political realism; there are, instead, many realist theories derived from the same first principles and basic set of assumptions. Taken together, this large and growing body of work forms a theoretical perspective that has achieved the status of dominant research program in the study of world politics. Like all research programs, political realism consists of an irrefutable "hard core" and a surrounding "protective belt" of auxiliary hypotheses that "bear the brunt of tests and get adjusted and readjusted, or even completely replaced, to defend the thus-hardened core" (Lakatos 1970, 133). In my view (see Schweller and Priess 1997, 6–8), the "hard core" of the realist school of thought consists not of Waltz's balancing proposition but of seven propositions (or, rather, assumptions) about international politics. (1) Humans do not face one another primarily as individuals but as members of groups that command their loyalty (Gilpin 1986, 304–5; 1996, 7). (2) International affairs take place in a state of anarchy. (3) Power is the fundamental feature of international politics (Morgenthau 1967); it is the currency of international politics required to secure any national goal, whether world mastery or simply to be left alone. (4) The nature of international interaction is essentially conflictual: "A world without struggle would be a world in which life had ceased to exist" (Spykman 1942, 12). (5) Humankind cannot transcend conflict through the progressive power of reason to discover a science of peace (see Morgenthau 1946, 90–5, chapter 8). (6) Politics are not a function of ethics; morality is the product of power (Carr [1946] 1964, 63–4; Spykman 1942, 18). (7) Necessity and reason of state trump morality and ethics when these values conflict (Wolfers 1962, 244).

The centrality of realism's hard core in the field of international relations is similar to that of rationality in the field of economics: It serves as *the* baseline expectation for empirical observations; that is, anomaly appears primarily, if not only, against the background of the realist research program. All other competing theoretical perspectives in international relations—for example, neoliberal institutionalism, bureaucratic politics, social constructivism, and so forth—have been defined in opposition to realism. When anomaly does appear, or ambiguity arises as the realist perspective is applied to new areas of interests, or when empirical or experimental work is undertaken to improve the explanatory and predictive precision and power of different auxiliary hypotheses, change takes place in the "protective" belt, but it must not occur in the hard core. Because the formulation of a new research program is a relatively rare event, devising and testing amendments to the program's protective belt of auxiliary hypotheses makes up the bulk of the work that scientists do. (In some ways, the development of a research program's auxiliary hypotheses can be seen as analogous to Thomas Kuhn's description of normal science [see Kuhn 1970, 33]). Thus, no one should be surprised, much less shocked or alarmed, to find that political realism has been continually revised, reformulated, and amended for the purposes of (1) better explanations and more determinate predictions, (2) refinement and clarification of the research program's theoretical concepts, and (3) extension of the research program to cover new issue areas.

Political realism is more than a "scientific" research program, however; it is also a political philosophy or worldview—one that is profoundly pessimistic about the human condition, moral progress, and the capacity of human reason to create a world of peace and harmony. These recurrent pessimistic themes, more than any specific proposition or testable hypothesis, make it possible to speak of a tradition of realist thought that includes Thucydides, Machiavelli, Hobbes, Rousseau, Weber, Kissinger, Waltz, and Mearsheimer. All political realists, whether philosophers or scientists, share this pessimism about human

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relations, which they depict as a perpetual struggle for security, prestige, and power and influence (control over territory, the behavior of other states, and the world economy).

In more technical terms, realists see a world of constant positional competition among groups under conditions of scarcity. By positional I mean that what counts is not the players' absolute skills or capabilities but how they perform relative to their opponents. In such situations, a change in the absolute capability of any actor (holding constant the remaining actors' capabilities) has important effects not only for that player but also for the other contestants (see Frank 1991; Schelling 1978, chapter 7). By competition I mean that the goal of the players is to win (viz., primacy; see Huntington 1993) or, at a minimum, to avoid relative losses (see Grieco 1990). Neorealism's assumption that states seek to maximize their security (not power or influence) transforms classical realism from a game of pure positional competition to one of collaboration with mixed motives. This is because, among security-seeking states, there is no inherent competition—no one winner (see Schweller 1996). In theory, security is a positive-sum value; it can, under certain conditions, be both commonly desired and commonly shared without diminishing its enjoyment for any individual actor. The same cannot be said for positional goods, such as prestige, status, political influence, leadership, political leverage, a positive trade balance, or market shares (see Hirsch 1976; Jervis 1993, 58–9). All states cannot simultaneously enjoy a positive trade balance; and if everyone has status, then no one does. Indeed, scarcity confers status (see Mishan 1982, chapter 17; Shubik 1971). Positional competition is therefore zero-sum: a gain (loss) for one player becomes a corresponding loss (gain) for the opponent(s). Here one is reminded of Francis I, who, when asked to list the disagreements underlying the perpetual warfare between him and his archrival Charles V, replied: "None whatever. We agree perfectly. We both want control of Italy!" (quoted in Waltz 1959, 187–8).

Turning to my own work, Vasquez maintains that I redefine "bandwagoning more broadly than Walt; it is no longer the opposite of balancing (i.e., siding with the actor who poses the greatest threat or has the most power) but simply any attempt to side with the stronger, especially for opportunistic gain" (p. 905). Vasquez acknowledges that I define bandwagoning as "any attempt to side with the stronger" but then claims that this is somehow different or inconsistent with "siding with the actor that . . . has the most power." Perhaps the key words are "any attempt." They are not my words, however. Like Waltz, I simply define bandwagoning as siding with the stronger state or coalition.[1]

It is Walt (1987), not I, who redefines bandwagoning. In order to retain the balancing/bandwagoning dichotomy and to make it fit his balance-of-threat framework, Walt (1987, 17) redefined bandwagoning as "alignment with the source of danger." As I pointed out (Schweller 1994), the problem with Walt's definition is that it (1) confuses bandwagoning with strategic surrender,[2] (2) defies conventional usage and the common meaning of the term, and (3) by viewing bandwagoning solely as a response to threat, ignores the primary motivation for bandwagoning, namely, the expectation of profit and easy gains. The more general point is that alliances are responses not only to threats but also to opportunities. By defining bandwagoning as a response to threat, Walt's theory overlooks the principal reasons states, particularly nonthreatened ones, would engage in this behavior.

Vasquez further claims that my "new concepts do not point to any novel theoretical facts" (p. 906). This charge would be true if I had simply confirmed the standard hypothesis that weak states sometimes ally with strong states when it is to their advantage. I did not do this, however. Instead, my bandwagoning argument and my cases focused on *great powers*, not weak ones; that is, I examined the behavior of precisely those states that realists claim are most likely to balance dangerous accumulations of power. Therefore, my conclusion that great powers typically bandwagon with rising dissatisfied challengers is, indeed, theoretically important and novel. I observed that England and the Habsburgs bandwagoned with Louis XIV's France 1667–79, when France achieved hegemonic status in Europe (Schweller 1994, 89–90). Similarly, during the Napoleonic wars, every great power (Prussia, Austria, Russia, Spain) with the exception of Great Britain bandwagoned with France at one time or another (1994, 90–2). Likewise, Japan, Italy, and the Soviet Union bandwagoned with Hitlerite Germany (1994, 94); and some scholars even suggest that France did so in 1940.

My argument is straightforward: Unthreatened revisionist states (those overlooked by Walt and Waltzian neorealists) often bandwagon with the stronger revisionist state or coalition for opportunistic reasons. My use of the term conforms to the standard definition of bandwagoning as joining the stronger side; and I show that this behavior is quite prevalent throughout history among a certain class of states (Schweller 1994, 1998).

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[1] In fact, the term "bandwagoning" as a description of international alliance behavior first appeared in Quincy Wright's *A Study of War* ([1942] 1964, 136) and only much later in Waltz (1979, 126; also see Wolfers 1962, 124). Both Wright and Waltz employ the concept to serve as the opposite of balancing behavior or what Wright ([1942] 1964, 136) called "the underdog policy." Bandwagoning refers to joining the stronger coalition; balancing means allying with the weaker side. Contrary to neorealist logic, however, Wright correctly points out that bandwagoning can, under certain circumstances, preserve the balance of power. This occurs when "the stronger in a given war is a relatively weak state whose strengthening is necessary to hold a more powerful neighbor in check" (Wright [1942] 1964, 136).

[2] Walt's bandwagoning is a preventive form of strategic surrender. The prospective loser agrees not to initiate hostilities and to transfer its residual military capability to the prospective winner in exchange for immunity of life, the avoidance of losses it would incur in a certain military rout, and (if the loser retains some bargaining assets) the possibility of political concessions, e.g., the survival of the loser's authority structure. Sometimes the weaker side capitulates rather than fights in order to conserve its strength for a future battle under more favorable conditions. By giving in without a fight, it gains a breathing spell, during which it expects the balance of power to shift against the more powerful aggressor. See Kecskemeti 1958, chapters 1 and 2.

This is an important theoretical insight that explains novel facts. I did not claim, however, that I had falsified the "balancing predominates" proposition. Instead, I wrote: "Balancing is an extremely costly activity that most states would rather not engage in, but sometimes must to survive and protect their values. Bandwagoning rarely involves costs and is typically done in the expectation of gain. This is why bandwagoning is more common, I believe, than Walt or Waltz suggest" (Schweller 1994, 93).

But even if I had shown conclusively that more states bandwagon than balance (a claim I did not make), this would not necessarily contradict or disconfirm Waltz's balancing proposition. Balancing forces may still prevail at the systemic level (that is, balances of power may recurrently form after their disruption), as Waltz's theory predicts. This is because satisfied great powers are typically the most powerful ones with the greatest interest in maintaining the status quo. They are therefore likely to balance against revisionist aggressors and to be successful, even when outnumbered.

Finally, Vasquez writes: "Schweller . . . argues that his theory is even more realist than Waltz's, because he bases his analysis on the assumption of the classical realists—states strive for greater power and expansion—and not on security, as Waltz . . . assumes" (p. 905). Here again, Vasquez is only partially correct. Contemporary realists, such as Waltz and Walt, treat *all* states as if they were satisfied, status-quo powers that seek primarily to maximize their security rather than their power. I argue that this status-quo bias overlooks the main protagonists or catalysts of balance-of-power theory: revisionist, dissatisfied powers that seek to expand their power at the expense of others. Without these states, there would be little need for security (or security studies) in world politics (see Schweller 1996). Consistent with traditional realism, therefore, my balance-of-interests theory includes *both* revisionist states (those that seek to increase their power) and status-quo states (those that seek merely to keep what they already possess). By relaxing neorealism's assumption that all states value what they possess more than what they covet, my theory allows for the full range of state interests.

The essential point is that, for neorealists, conflict and competition are framed in terms of self-preservation, that is, states fear being dominated or even destroyed by others. As a result, neorealism is relatively silent about other important state goals, such as "tranquility, profit, and power," which Waltz (1979, 126) views as secondary interests that can only be safely sought "if survival is assured." History, however, shows otherwise (see Zakaria 1997). In the words of fellow realist, Raymond Aron (1966, 598): "All great states have jeopardized their survival to gain ulterior objectives. Hitler preferred, for himself and for Germany, the possibility of empire to the security of survival. Nor did he want empire—or an accumulation of power—as a means to security." Even if we concede Waltz's point that survival is the sine qua non for the pursuit of other goals, the question arises: When survival is assured, what does neorealism explain? Not enough, in my view; that is why I developed balance-of-interests theory.

By highlighting apparent theoretical contradictions and discrepant patterns of evidence within the realist research program, Vasquez's appraisal of neotraditional research serves a useful purpose. Yet, it seems to me that the problem is not one of a degenerating research program but of underspecified scope conditions. Realists, like most other theorists in the discipline, have done a poor job of identifying the conditions required for their theories to operate; as Bobrow (1972, 15) put it, "one reason for conflicting generalizations is the failure to state the necessary conditions for one rather than the other to hold." With regard to the balancing proposition, I agree with Walt's conclusion that states typically respond to threats by balancing against, not aligning with, the source of danger. But because states align for reason other than security, I disagree with his more general claim that states usually balance and rarely bandwagon. As I wrote: "Walt does not offer a theory of alliances so much as a theory of how states respond to external threats" (Schweller 1994, 83). Both his theory and his "balancing predominates" proposition apply only to threatened states. In contrast, I looked at how unthreatened states respond to opportunities in their environment and found that bandwagoning is a common form of behavior, especially among dissatisfied states. The two findings are not inconsistent. When proper attention is devoted to the scope conditions of our theories, much of what appears to be contradictory and discrepant proves consistent and complementary.

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The Progressive Power of Realism

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John Vasquez's assessment of realism suffers from three serious flaws. First, his reliance on Imre Lakatos's (1970) model of scientific progress is problematic, because the Lakatosian model has been largely rejected by contemporary historians and philosophers of science. Second, Vasquez understates the range and diversity of the realist research program and mistakenly sees disagreements among realists as evidence of theoretical degeneration. Finally, he overlooks the progressive character of contemporary realist theory, largely because he does not consider all the relevant literature. Disagreements within and across competing research programs are essential to progress and should be welcomed, but Vasquez's effort suggests that criticism will be most helpful when it seeks to do more than merely delegitimate a particular research tradition.

John Vasquez's evaluation of the realist research program is a misstep on the road to better international relations theory. By portraying realism as "degenerating," Vasquez hopes to influence both "individual decisions about where scholars are willing to place their research bets, *as well as collective decisions as to which research programs deserve continued funding, publication, and so forth*" (p. 900, emphasis added). In other words, his main goal is to discredit realism as a legitimate approach to the study of world politics, discourage scholars from pursuing a realist research agenda, and make it less likely that scholars working in the realist tradition will receive research funds or access to prominent journals.

What evidence does Vasquez present to justify this extreme position? Vasquez's (1997) criticism of realism is based on one philosopher of science (Imre Lakatos) and a handful of contemporary realist writings. Specifically, to support his charge of "degeneration," he has selected two "realist" books (Waltz 1979, Walt 1987), two articles (Schweller 1994, Christensen and Snyder 1990), and an eleven-page letter to the editor (Elman and Elman 1995). Does such a cursory survey justify discarding a respected intellectual tradition? Would we consider abandoning rational choice theory, the cognitive approach to decision making, organization theory, liberal theory, or the quantitative analysis of voting behavior on the basis of a similar sample? Even if every one of Vasquez's criticisms were valid (which is not the case), his evaluation would say little about the value of the realist perspective.[1]

The foundation of Vasquez's critique is Lakatos's (1970) well-known essay on scientific research programs. Lakatos argued that a research program consists of (1) a "hard core" of basic propositions accepted by all members of the research community, (2) a "negative heuristic" that deflects criticism away from this hard core, and (3) a "positive heuristic" that identifies legitimate puzzles and sets the research agenda. According to Lakatos, a research program is "progressive" if new theoretical refinements lead to "excess empirical content" (i.e., to newly confirmed predictions) when compared with the earlier theory. By contrast, a research program is said to be "degenerating" if each new theory is merely an ad hoc or semantic adjustment that explains an anomaly but does not anticipate some "novel fact."

Influenced by the Lakatosian model, Vasquez portrays realism as a narrow, tightly unified research program that is exhibiting clear signs of degeneration. After describing what he takes to be realism's hard core, he argues that recent theoretical refinements are merely ad hoc adjustments designed to rescue the entire paradigm from its purported empirical failings. He does this by showing that a handful of realists have advanced different theories about alliance formation. Vasquez sees these disagreements as a symptom of degeneration, because the presence of several competing realist theories increases "the probability that the realist paradigm will pass some test" (p. 906).

There are three main problems with Vasquez's criticism. First, his reliance on Lakatos is problematic, both because that model of scientific progress is flawed and because Vasquez's interpretation of it would justify abandoning most (if not all) of social science theory. Second, Vasquez's characterization of the realist tradition is misleading and understates its range and diversity. Third, Vasquez overlooks the progressive character of contemporary realist theorizing, in large part because he did not consider all the relevant literature. In particular, his treatment of my own work is both inconsistent and demonstrably inaccurate. Taken together, these errors explain why his article leaves realism largely unscathed and sheds little light on how it might be improved.

HOW NOT TO JUDGE SOCIAL SCIENCE THEORIES

Vasquez relies on Lakatos's model of scientific progress because he believes that it "has attracted the most consensus" as a source of criteria for judging theoretical merit, at least among mainstream scholars

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[1] As discussed below, Vasquez's sample is not even a comprehensive survey of the relevant works on the (relatively) narrow subject of balancing behavior.

of international relations (pp. 899–900). Even if this assertion were correct, however, it would not be a persuasive justification. Lakatos's now-dated analysis has been largely rejected by contemporary historians and philosophers of science (Diesing 1991, Laudan 1977, Suppe 1977, Toulmin 1972). Why should social scientists embrace a model of scientific progress that has been widely discredited by experts in that field?

Lakatos's analysis has been discarded mainly because it does not square with what we now know about scientific discovery. For example, Lakatos argues that scientists in a particular research program share a "hard core" of common assumptions, but the historical record shows that scientists working in such a program often disagree about its central elements.[2] Similarly, Lakatos's model implies that progress takes place only at the research frontier, while the hard core of a research program remains unchallenged. In fact, the hard core is often the object of debate, and it evolves in response to new empirical discoveries and conceptual innovations. Thus, Lakatos's model of scientific progress is doubtful on purely historical grounds.

Second, although Lakatos emphasizes that the key criterion in choosing between rival theories is "excess empirical content," he never explains how to perform this sort of comparison. For Lakatos, theory T_2 is progressive if it explains everything that the old theory T_1 did, while simultaneously accounting for some unanticipated "new fact." Yet, comparing the empirical content of rival theories turns out to be especially difficult in practice, which helps explain why "neither Lakatos nor his followers have been able to identify *any* historical case to which the Lakatosian definition of progress can be shown strictly to apply" (Laudan 1977, 77; see also Grunbaum 1976a; McCloskey 1994, chapter 7). A measure of progress that is difficult to operationalize is not a useful guide.

Third, Lakatos's rejection of ad hoc adjustments is inconsistent with actual scientific practice. An ad hoc adjustment that resolves an existing anomaly but does not lead to any other new facts is still an advance in our understanding; after all, it does answer a puzzle. Such an adjustment is problematic only if it simultaneously creates additional conceptual or empirical difficulties. Similarly, a refinement that limits the domain of a theory (i.e., by showing that it only operates under circumscribed conditions) is still an improvement over the prior but incorrect claim that the theory possessed a broader explanatory range. Thus, working scientists routinely embrace ad hoc adjustments and correctly regard them as part of normal scientific procedure.[3]

These points suggest that Lakatos's model is not a sound basis for judging realism or any other research program. Indeed, taken to its logical conclusion, Vasquez's application of Lakatos would justify abandoning virtually all social science theory. Vasquez sees a research program as degenerating if (1) new theories offer no new empirical content and (2) if the program "goes through a number of theoryshifts . . . the end result [of which] is that collectively the family of theories fields a set of contradictory hypotheses which greatly increase the probability of at least one passing an empirical test" (p. 901). As we shall see, his claim that certain realist theories have not produced "new facts" is simply wrong. Moreover, his claim that the emergence of contradictory hypotheses is a sign of degeneration in effect means that when scholars in a particular research program advance different theories or reach different conclusions, then that research program is ipso facto beginning to degenerate. This conclusion ignores the possibility that one scholar is correct and another is incorrect, as well as the possibility that a particular disagreement can be reconciled by empirical testing or by a more precise specification of intervening variables or boundary conditions (Schweller 1997). Adopting this standard would force us to reject virtually every research tradition in the social sciences.

WHAT IS REALISM?

Vasquez appears to regard realism as a single, tightly unified research program, centered around the ideas of Kenneth Waltz. This view leads him to see any major disagreement among realists—and especially any departure from Waltz—as a sign of degeneration, which in turn leads him to portray other realists as trying to salvage the larger paradigm through a series of ad hoc amendments. This perspective also allows him to count the discrediting of any particular realist theory as a blow against the entire paradigm.

Vasquez's view rests on an inaccurate picture of contemporary realist thought. In fact, realism is a broad research program that contains a host of competing theories. Realists begin with some general assumptions (such as states are the key actors, the international system is anarchic, power is central to political life).[4] As with all successful research programs, however, realists also disagree about a host of fundamental ideas. For example, Hans Morgenthau assumes that competition between states arises from the human lust for power (which he termed the animus dominandi), while Kenneth Waltz ignores human nature and assumes that states merely aim to survive (Morgenthau 1946, Waltz 1979). "Offensive" realists, such as Mearsheimer (1994–95), argue that great powers seek to maximize security by maximizing their relative power, while "defensive" realists, such as Jack Snyder (1991) or Charles Glaser (1994–95), argue that great powers are generally more secure when they refrain from power maximization and seek to defend the status quo. Realists also disagree about the relative importance of domestic versus systems-level causes, the relative stability of bipolar versus multipolar worlds, and the importance of intentions in shaping the calculations of national leaders (to name but a few possibilities). Thus, far from being a narrow intellectual monolith, realism is a large and diverse body of thought whose proponents share a few important ideas but disagree about many others.[5]

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[2] To note one example, both Charles Darwin and Alfred Wallace worked within the Darwinian "research program," insofar as each believed in the core principle of natural selection. Yet, they disagreed on several basic issues, such as the inheritability of acquired characteristics (Richards 1987, chapters 2 and 4).

[3] As Laudan (1977, 115) puts it: "If some theory T_2 has solved more empirical problems than its predecessor—*even just one more*—then T_2 is clearly preferable to T_1 and, *ceteris paribus*, represents cognitive progress with respect to T_1 . . . . Ad hoc modifications, *by their very definition*, are empirically progressive." See also Grunbaum 1976b.

[4] Some realists might add the assumption that states are more or less rational actors, although several prominent realists (including the present author) also examine how domestic politics can affect the "rational" assessment of strategic interests. For a sample of recent attempts to identify the core features of the realist paradigm, see Mearsheimer 1994–95; Van Evera, n.d., chapter 1; Walt 1992b, 473.

Two implications follow. First, it is hardly evidence of degeneration when realists advance contradictory arguments or reach different conclusions, just as it is not a major issue whenever neo-Keynesian economists, Skinnerian psychologists, Darwinian sociobiologists, or quantum physicists are at loggerheads. There are a host of competing theories within the realist paradigm, and not all of them are going to be equally valid or useful. Second, the failure of a particular realist theory does not discredit the entire paradigm, especially since realism deals with a very wide variety of international phenomena. Vasquez focuses on a handful of authors in his attempt to discredit the entire approach, but this step mischaracterizes the broader research tradition and the many different theories it contains.[6]

The real question to ask is whether realism—with all its limitations—has advanced or impeded our understanding of international relations. On this issue, even well-known critics of realism concede that it has been an influential tradition precisely because it sheds considerable, if only partial, light on a number of important international phenomena (Keohane 1984, 1986; Ruggie, 1983; Wendt n.d.).[7]

POWER, THREAT, AND EMPIRICAL CONTENT

The problems with Vasquez's analysis are evident in his discussion of my own work. To begin with, he cannot make up his mind about the theoretical status of balance-of-threat theory. He begins by portraying my theory as a direct refutation of Waltz's neorealist balance-of-*power* theory, based on my assertion that states tend to balance against *threats* rather than against power alone. In his view, this challenge to Waltz has "devastating" consequences for the realist paradigm. As Vasquez puts it, "if . . . power and threat are independent, as they are perceived to be by the states in Walt's sample, then something may be awry in the realist world" (p. 904).

Yet, it is hardly clear why refuting Waltz would lead us to abandon the realist paradigm in toto. Vasquez clearly regards my work as part of the realist paradigm, so if I have correctly refuted Waltz, then realism is a progressive program after all. To avoid this obvious challenge to his argument, Vasquez reverses course and argues that balance of threat theory is merely a "felicitous phrase" that "makes states' behavior appear much more consistent with the larger paradigm than it actually is." In particular, he claims my theory "does not point to any novel facts other than the discrepant evidence [and] . . . . does not have any excess empirical content compared to the original theory, except that it now takes the discrepant evidence and says it supports a new variant of realism" (pp. 904–5). In short, Vasquez begins by calling balance-of-threat theory a "devastating" challenge to Waltz, based on the claim that power and threat are wholly independent concepts. But he quickly backtracks to argue that balance-of-threat theory is merely a semantic repackaging of Waltz's theory that does not point to any novel facts. He cannot have it both ways.

As it turns out, both assertions are incorrect. With respect to the first, I do not see power and threat as independent. Balance-of-threat theory openly incorporates power, subsuming it (along with geography, offensive capabilities, and intentions) within the more general concept of *threat*.[8] Balance-of-*power* theory predicts that states will ally against the *strongest* state in the system, but balance-of-threat theory predicts they will tend to ally against the most *threatening*. Thus, the latter can explain not only why a state may align against the strongest power (if its power makes it the most dangerous) but also why one state may balance against another state which is not necessarily the strongest but which is seen as more threatening on account of its proximity, aggressive intentions, or acquisition of especially potent means of conquest. The two theories are not the same, although they share certain elements.

With respect to the second assertion that balance-of-threat theory is merely a semantic repackaging, I point to several of my works that offer novel facts. Balance-of-threat theory was originally laid out in my 1987 book, which examined alliance behavior in the Middle East. The final chapter showed, however, that it can also explain the anomalous distribution of power between the Soviet and American alliance systems during the Cold War. In a subsequent article (not cited by Vasquez), the theory was used to explain the alliance behavior of four different states in Southwest Asia (Walt 1988). Another article (also not mentioned by Vasquez) shows how balance-of-threat theory explains alliance dynamics in Europe during the 1930s, a period that poses an especially demanding test for the theory. In particular, the theory explains why (1) the East European states failed to balance effectively against both Nazi Germany and the Soviet Union, (2) the United States was the last great power to mobilize for World War II, and (3) Great Britain and France balanced more slowly than hindsight might dictate. Parenthetically, this article also shows that Britain and France did not fail to balance the rising threat from Nazi Germany, as Vasquez, Schroeder, and others imply (Walt 1992b). Other scholars have successfully used balance-of-threat theory to explain the formation of the Gulf War coalition in 1990–91 and to analyze the grand strategy of the United States in the post–Cold War period (Garnham 1991, Mastanduno 1997). Finally, Vasquez does not refer to my recent efforts to apply the theory to a new realm—the international consequences of domestic revolutions (Walt 1992a, 1996). Thus, Vasquez's central claim—that balance-of-threat theory does not have "excess empirical content"—is false. And with this error exposed, his argument collapses.

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[5] Recent examples and discussions of the broad body of realist thought include Brooks 1997; Brown, Lynn-Jones, and Miller 1995; Desch 1996; Deudney 1993; Elman 1996; Frankel 1996a, 1996b; Gilpin 1986; Grieco 1990; Van Evera n.d., chapter 1; and Zakaria 1992.

[6] The most egregious example is Vasquez's claim that Colin and Miriam Elman's 1995 letter to the editor of *International Security* illustrates the response to Schroeder of scholars "sympathetic to realism" (p. 908). Such an assertion would be valid only if realism were in fact a single theory and if all so-called realists agreed with the Elmans' position. Vasquez offers no evidence that this is the case, which it is surely not.

[7] For example, realism provides cogent explanations for (1) the failure of all modern efforts to gain hegemony over the state system; (2) the nearly universal tendency for great powers to be extremely sensitive to shifts in the balance of power; (3) the constancy of security competition among great powers; (4) the difficulty of sustaining effective international cooperation; (5) the tendency for great powers to acquire either formal empires or informal spheres of influence; and (6) the tendency for great powers to imitate one another other over time. Realism does not provide the *only* explanation for these (and other) phenomena, but it contains a set of explanations that one would not want to dismiss out of hand.

[8] As I wrote in *The Origins of Alliances*: "Balancing and bandwagoning are usually framed solely in terms of capabilities. . . . This conception should be revised, however, to account for the other factors that statesmen consider when deciding with whom to ally. *Although power is an important part of the equation, it is not the only one*" (Walt 1987, 21, emphasis added; see also 263–4).

CONCLUSION

Viewed as a whole, Vasquez's essay is a classic illustration of the hazards of small sample size. First, he relies on one modern work on the history and philosophy of science. Second, he relies on five contemporary realist works. Finally, he cursorily surveys the writings he examines, thereby missing the novel facts they uncover. This combination of problems is fatal to his argument, and prevents him from offering a useful criticism of realism in general or the specific body of literature under examination.

This failure is unfortunate, because realism is not without flaws and certainly should be exposed to criticism. The realist perspective offers a simple and powerful way to understand relations among political groups (including states) and offers compelling (albeit imperfect) accounts of a diverse array of international phenomena. But it is hardly the only way to study international relations. In the future, as in the past, scholars will continue to revise and extend the diverse body of realist thought. In doing so, they will inevitably disagree in various ways. At the same time, other scholars will pursue a variety of nonrealist research programs, and the resulting competition among different approaches will help us refine our understanding of international politics. The clash of theories both within and across competing research programs is essential to progress in the social sciences and should be welcomed. Progress will be swifter, however, if criticism seeks to do more than merely delegitimate realism, or any other approach a critic happens to dislike.

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BOOK REVIEWS

Political Theory

The Dissonance of Democracy: Listening, Conflict, and Citizenship. By Susan Bickford. Ithaca, NY: Cornell University Press, 1996. 204p. $39.95 cloth, $14.95 paper.

David Kahane, *University of Alberta*

In this lucid and intelligent book, Susan Bickford offers an account of democratic deliberation meant to show the political significance, as well as the interconnection, of group memberships and individual opinions. Whereas many democratic theorists focus on the nature of good reasons or the conditions necessary for uncoerced consensus, Bickford highlights the qualities of listening required to bridge misunderstandings fostered by unequal relations of social power and to allow collective action amid the dissonances that remain. Drawing upon Aristotle, Arendt, Merleau-Ponty, and contemporary feminist theory, she contributes to attempts by deliberative democrats to make theoretical and institutional space for group-specific oppressions, interests, and identities while also attending to the complex heterogeneity of social groups.

Bickford turns to Aristotle for an account of political deliberation as requiring attention to the variety of perspectives in the state, but she shows how he tends to read these perspectives in terms of material conditions, mitigating the need actually to listen to other citizens. She uses Arendt to depict public action as a process of self-creation and transformation; possibilities for understanding and alliance cannot be derived from the preexisting opinions or interests of groups. Furthermore, the vitality of the public sphere requires that we recognize the individual distinctiveness made manifest through public speech and action.

Bickford's focus is on a tension between recognizing the relevance of social position to public identity and hearing "plurality"—the distinctive life story and set of opinions embodied in individual voices. Arendt insists that to preserve plurality, the public sphere has to allow citizens to express *who* they are without reference to *what* they are: Politics must be insulated from the material conditions of "the social." Bickford modifies Arendt: While our opinions are our own, they are expressed in vernaculars that reflect our group memberships and are interpreted in terms of hierarchical relations of group power. "The content of my speech may disclose a distinct personal identity, but my way of speaking may also point to a specific social, cultural, racial, and gendered identity" (p. 96).

Drawing upon a range of feminist work on social location and public identity, Bickford argues that putatively neutral norms of communication in fact privilege powerful groups' speech structures, voice qualities, affective dispositions, and ways of framing statements. Actual patterns of speech combine with stereotypes to keep the opinions of members of oppressed groups from being heard. Iris Young has made a similar point, arguing that democratic institutions should give an explicit place to group-specific perspectives through representation, vetoes, and so on. But Bickford, like many other critics, argues that Young neglects the multiplicity and indeterminacy of identities and interests, and so courts an identity politics that would subsume plurality to reductive group definitions. Bickford does not wish to treat group identities as a fiction, but she does want to mobilize group identities in ways that preserve a public place for plurality. She draws on the work of several feminists of color, particularly Gloria Anzaldúa, to describe a multiple-voiced subjectivity that acknowledges its origins while refiguring identity in public space: "This understanding gives rise to the possibility of a public identity that is more than just a string of labels, yet does not ignore the relevance to our lives of the groups those labels name" (p. 125).

She then offers a nuanced philosophical discussion of listening, showing how it maintains one's own perspective as background while focusing on the situation and opinions of another: The point is not to merge voices but to hear divergent perspectives in relation to one another. An awareness of this dissonance can clarify the nature of conflicts, and the very fact of recognizing divergent perspectives can guide collective action while fostering a commitment to continuing the conversation.

Bickford successfully shows how listening, as a particular form of reflexivity and understanding, can make deliberative space for the perspectives both of groups marginalized by dominant discourses and of individuals marginalized by the language of "groups." But there is a difficult dialectic between the political assertion of marginalized group perspectives and attentiveness to the diversity of the individuals constituting these groups. Bickford remains too close to Arendt in her stress on plurality, thereby drawing her argument away from the exigencies of politics. This is a matter of emphasis: In her discussion of poststructuralist feminism, Bickford explicitly leaves space for the political assertion of social identities. But more often than not, she worries about group memberships obscuring individual distinctiveness: "If my membership in a group is the only lens through which I am perceived, then I cannot appear as a person with a unique story and singular opinions. The expression of human plurality is blocked by assumptions that I am simply a representation of others who look and sound like me" (p. 101).

There are political contexts in which justice demands a focus on individual distinctiveness and on the threat posed by stereotypes or group labels. Anzaldúa, for example, contests invocations of group solidarity that would force her to choose one aspect of her identity over others or draw lines between parts of her identity. Here, the stifling of plurality indicates that a group definition is impositional in ways that may demand political attention (some readers may agree with Bickford that failing to hear individual opinions is itself a political harm; others may see the stifling of plurality by group definitions as a marker of possible injustices to subgroups).

In other contexts, however, resistance to oppression requires strong generalizations about group-specific interests and perspectives. Some of Bickford's own examples inadvertently bring out the importance of invocations of an oppositional collectivity: The students who wonder whether to use black English in their letter challenging police brutality seem comfortable with generalizations about their interests and perspectives as compared to those of the police (pp. 127–8). They may find a moment, later, for questioning whether they all really do mistrust the police; whether their diverse genders, classes, and sexualities complicate stories about their collective identity; or whether they are missing opportunities for alliances with members of the police force; in writing the letter, though, they legitimately may stick with a generalization like "blacks have good reason to mistrust the police." Such a generalization brings out politically salient patterns, without insisting that every member of the group share a single characteristic (see Cressida Heyes, "Back to the Rough Ground: Wittgenstein, Essentialism, and Feminist Methodologies," in Naomi Scheman, ed., *Rereading the Canon: Feminist Interpretations of Ludwig Wittgenstein,* 1998); the generalization is important in contesting an injustice even as its imprecision risks imposing new exclusions and injustices.

Listening has a crucial role to play both when collective identities are asserted and when they are questioned or challenged, but it may not play the same role or take the same form in the two political moments. Insofar as we want to trace the political implications and institutional conditions of good democratic listening, we need to explore strategies for asserting a "we" (say, for purposes of representation or mobilization), for interrogating a "we," and for deciding when to do one or the other (e.g., see William E. Connolly, *The Ethos of Pluralization,* 1995). Bickford gives us an important start on this project, but to understand how listening may inform democratic politics in its breadth, we need to look more carefully at particular cases, supplementing her emphasis on plurality with attention to the politics of generalization and to the dialectic between these two aspects of collective action.

Privacy and the Politics of Intimate Life. By Patricia Boling. Ithaca, NY: Cornell University Press, 1996. 192p. $37.50 cloth, $15.95 paper.

Women, Politics, and Reproduction: The Liberal Legacy. By Ingrid Makus. Toronto: University of Toronto Press, 1996. 274p. $55.00 cloth, $19.95 paper.

Eileen Bresnahan, *University of Utah*

Although these books cover some of the same ground—both discuss issues related to abortion and to arguments for the transformation of politics in terms of an ethic of care—they do so in different ways, from very different perspectives, and with widely divergent degrees of success. Boling's is a feminist reconsideration of the dangers and the value of privacy, animated by her own conflicted feelings about this concept. Agreeing that privacy both obscures power and oppression and also protects some of the most valuable and vital areas of human experience and personality, Boling critically questions the extent to which "the personal is political," examines distinctions between what is private and what is political, and argues that the maintenance of this distinction is necessary if either politics or intimate life are to be fully human. She discusses Hannah Arendt, the privacy needs of families and of gays, "outing," identity politics, and problems in the language and in the legal definitions of privacy, in an interesting and nuanced attempt to sort out why privacy deserves our respect. As a result, her account is constructed of tensions and complexities, making it an engaging way to revisit in the context of gender and sexuality a familiar issue, as well as a suitable book for introducing students to some of the problems in both privacy and modern politics.

I must, however, take exception to Boling's claim that one of the central virtues of her work is its description of how issues of private life—by being transposed into common political language and delivered in the proper political "spirit"—can be transformed into political issues. Specifically, what troubles me is the reductive optimism that creeps in when Boling discusses the politically transformative possibilities of "mother's issues." This discussion is largely detached from considerations of power and interest in maintaining the status quo of gendered relations, arguing that if women and women's concerns can just be taken seriously by those in power—and essentially proposing that if we can just abolish gender roles and move on from there—things will be very different. Indeed, they would be, but is not that the sticking point? Like the old slogan/question, "What if they had a war and nobody came?" such theorizing skips over the hard questions of transformation in a context of socially sanctioned power imbalances, in favor of imagining our instantaneous reincarnation into a society of angels. Surely, this is nothing that legions of political theorists have not done before, but I was disappointed to find it in such a generally honest work.

Makus, in contrast, is concerned not with privacy but with autonomy (I agree with Boling that the former is not merely an aspect of the latter) and less with ambiguity than with establishing a correct reading of canonical liberal texts. Her work is aimed at setting right the erroneous "feminist" view that liberals exclude women from their models of political community because they believe women lack rationality. Instead, Makus contends that women are excluded *precisely because* they are seen as rational and capable of acting in response to their own self-interest, and also that it is because of parent-child relations (rather than adult female-male relations, on which she believes feminists have too greatly focused) that this exclusion is seen as necessary by liberals. In essence, Makus argues that women are subordinated in liberal society for reasons of reproduction. Because liberals see the bearing and rearing of children as advancing important political ends—population regulation, the generational continuity of the political community (through education in citizenship to create consenting adults), and the accumulation of excess property—they hold the self-interest of women more important to the survival of the social and political order than that of men. What scares liberal political theorists about women's equality is the idea that women may use their rational self-interest to ignore their duties to reproduction and to the family and consequently may damage what is alleged to be the general well-being of society. On the one hand, Hobbes and Locke—although seeing men and women as mostly alike in reason, self-interest, and political capacities, and often viewing women's social inferiority as more a matter of custom than of inferior capacities—address this fear by making wives in civil society subject to their husbands. On the other hand, J.S. Mill—although he explicitly argues that women should have the same rights and opportunities as do men—assures his readers that though women be given equal rights, they will continue to care selflessly for families because they are different from men, granting women formal equality of opportunity not likely to translate into equal outcomes.

Makus contends that presently Hobbesian, Lockean, and Millsian alternatives "are assumed or invoked to regulate, maintain, and account for links between generations" (p. 130). Hobbes's model leads to a view of the parent-child relationship as contractual and as requiring extensive state intervention and support; Locke's to a belief in our divinely ordained duty to bear and rear children in order to ensure generational continuity; and Mill's to the idea that women have a greater propensity than men to care for children, to see their own interests as identical to those of their families, and in general to be more caring and nurturing of others. In contemporary debates over reproductive freedom, "Hobbesian self-interest" guides pro-choice arguments, while "Lockean duty" informs the pro-life; meanwhile, feminist discourse has become preoccupied with a Millsian "ethic of care," a development Makus finds disturbing, given liberal society's clear willingness to assign to women "the 'costs of caring' " (p. 156).

Although Makus's work is useful in providing another perspective on Hobbes's and Locke's views of women's rationality, it suffers the enormous problem that, because her new explanations for women's exclusion unquestioningly assume male dominance at their basis, they really do not explain very much at all. For example, one of her central contentions—that Hobbes and Locke see mothering as rational instead of natural and so fear granting women self-interest because this might disrupt generational continuity—is undercut by the acknowledged fact that liberals find it "difficult to show that mothering *or fathering* is in the parents' interests" (p. 167, my emphasis). Why, then, are women subordinated to ensure their reproduction, while men are provided incentives connected to power and control over women and children to compensate them for their reproductive trouble? Makus's answer would seem to be that liberals believe women's labor is more important than men's to the propagation of the species, because women can choose to terminate a pregnancy. But how is this problem better solved by making women subordinate to men than men subordinate to women? Might not a social structure in which women bore children and men reared them (without compensation, besides providing numerous personal services and domestic labor) alter women's incentive structure relative to reproduction at least as much as their subordination does?

An assumption of male dominance is also required by Makus's contention that liberals' exclusion of women has more to do with parent-child relations than with male-female relations. Certainly, liberals want to ensure generational continuity, but might this be done only by their methods? The methods liberals have chosen have *everything* to do with adult male-female relationships, in which reproductive labor is but one form of uncompensated and generally unrecognized labor that women provide to men and to the liberal state. Makus seems to have missed that liberals want to ensure the continuity of the political community only in ways that also maintain male dominance. She argues that women are subordinated in liberalism because their self-interest might frustrate the political desire to ensure reproduction, but she fails to see that their subordination secures this in a way organized from the outset by a presumption of male dominance.

The above problems may well be related to Makus's casual familiarity with feminist theory (most apparent in her last chapter on feminist alternatives). Certainly, the idea that women's subordination is tied to our reproductive capacity is not at all new, and it would have helped had Makus tried to locate her work in the feminist debates. Instead, she apparently does not see herself as participating in those debates but as beyond them, choosing to create a novel recategorization of leading feminist theorists that appears either ignorant or willfully dismissive of many of the theoretical affinities and central preoccupations that have lead to the more common taxonomy. Had she sought to understand what motivates feminist inquiry and to become more than passingly familiar with the complex body of work that makes up feminist theory, perhaps she would not have constructed such a naive and circular account.

This unfamiliarity with feminist theory is not a problem for Boling, who demonstrates a sophisticated understanding of feminist perspectives and politics. Those who wish an introduction from this angle to some of the issues related to abortion and maternal thinking will find much of value in her book. Though Makus may provide those concerned with Hobbes, Locke, and Mill new insights into their views on women, I would advise the uninitiated to take her attempts to explain and critique feminist theory with a large grain of salt.

Reasonable Democracy: Jürgen Habermas and the Politics of Discourse. By Simone Chambers. Ithaca, NY: Cornell University Press, 1996. 250p. $39.95 cloth, $16.95 paper.

William E. Scheuerman, *University of Pittsburgh*

Simone Chambers has written an extremely learned, carefully argued defense of Jürgen Habermas's theory of communicative rationality as a basis for conceptualizing deliberative democracy. Those interested in contemporary continental political thought will be hard-pressed to find a more cogent exposition of the enormously complicated twists and turns of Habermas's social and political theory over the course of the last twenty years, let alone any competing account that does as good a job of relating his ideas to contemporary North American debates in political theory. Chambers's book captures the nuances of Habermas's ideas *without* getting bogged down in the jargon and turgid prose characteristic of much of Habermasian scholarship. Needless to say, this is an impressive scholarly achievement.

Chambers begins by making the seemingly trivial assertion that "the more we employ noncoercive public debate to resolve our deepest collective moral, political, and social disputes, the better" (p. 1). By the work's conclusion, Chambers has made a plausible case that the everyday intuition that open dialogue provides the best means for grappling with political and moral conflict is most richly articulated by Habermas's model of communicative rationality, and a derivative conception of democracy according to which decision making is legitimate only when it rests on a process of unhindered, free-wheeling deliberative exchange. Although many scholars have similarly sought to defend Habermas in recent years, Chambers's effort is superior to theirs in at least three respects. First, she provides a refreshingly accessible discussion of the idiosyncrasies of Habermas's proceduralist and deontological moral and political theory, in which she thematizes his debt to Kant while rightly emphasizing the important ways in which Habermas breaks dramatically with much of Kantianism. (This element of the book makes it particularly appropriate for graduate level courses in contemporary political theory.)

Second, Chambers offers an excellent sustained comparison of Habermas and liberal theorists like Rawls and Scanlon. Although emphasizing the commonalities underlying Habermas and the most sophisticated recent versions of liberal theory, Chambers suggests that only Habermas's theory can succeed in resolving immanent problems within Rawls and Scanlon: Habermas alone succeeds in overcoming the internal tensions of contemporary liberal theory.

Third, Chambers does a good job of arguing that Habermas's political theory is superior both to competing rational choice and postmodern interpretations of democratic legitimacy. The former mistakenly posits that strategic action suffices to ground and stabilize democracy, whereas the latter obscures the crucial difference between coercion and persuasion by conceiving of deliberation as inevitably entailing willful, arbitrary power. Although adherents of neither orientation are likely to find her criticisms convincing, she formulates a set of arguments which they will have to take seriously.

Despite these strengths, it is hard to avoid the conclusion that Chambers's work suffers from an ambiguity endemic to much recent Habermasian scholarship. On the one hand, Habermas's conception of discourse ethics is supposed to constitute the starting point for a *critical* theory of society, capable of justifying substantial reforms of contemporary capitalist democracy. Particularly in his earliest discussions of the idea of communicative rationality, its role as a normative starting point for a radical critique of so-called late capitalism and the pervasive apathy and privatism of contemporary liberal democracy loomed large (Jürgen Habermas, *Legitimation Crisis,* 1973). On the other hand, Habermas has tended to moderate his political passions in recent years, while simultaneously reformulating many of his original ideas about communicative rationality and democratic politics; he now is skeptical of the viability of radical democratic calls for increased participation, and he has distanced himself from the traditional socialist critique of capitalism that played such an important role in his early work. Even those sympathetic to Habermas have worried that his recent contributions have abandoned too much of the critical agenda that once made his project so appealing to young left-wing political and social theorists.

Chambers's defense of Habermas reveals a similar ambivalence. On the one hand, Chambers never tires of asserting that free-wheeling debate à la Habermas is incompatible with coercion and many forms of inequality. On the other hand, *how much* of a challenge this proviso in fact poses to the pervasive inequalities of contemporary society remains unclear. Chambers is somewhat dismissive toward those, like Nancy Fraser, who have argued that Habermas's model of deliberative democracy fails to take the problems of gender and class inequality seriously enough (pp. 206–7). In a revealing choice of words, Chambers states that her main concern is with situating noncoercive debate "within the *existing* institutional framework of liberal democracies" (p. 10, my emphasis). Consistent with this, she has little to say about the massive problems posed to reasonable deliberation by contemporary capitalism, and her minimal suggestions for political reform could easily have come from those for whom critical theory is anathema (p. 196). Indeed, her attempt in the book's concluding section to apply Habermas's theory potentially obscures the profound limits on deliberation within contemporary society. According to Chambers, some crucial improvements in ties between French- and English-speaking Quebecers, based on relatively rich "communicative ties that bind participants" (p. 245), can be adduced as evidence for the superiority of Habermas's model of deliberative democracy. Even if we bracket issues raised by Chambers's surprisingly positive interpretation of recent Canadian developments, another problem presents itself: If political debate within present-day Canada constitutes an illustration of Habermas's theory, then what exactly is so "critical" about that theory? Of course, we all prefer that the Canadians continue to engage in debate rather than shoot each other. By the same token, Chambers's interpretation and application of Habermas suggests a striking deflation of the ambitious political energies that once motored Habermasian critical theory.

Public Morality and Liberal Society. By Harry M. Clor. Notre Dame, IN: University of Notre Dame Press, 1996. 235p. $32.95.

Dennis G. Stevens, *Augustana College*

In opposition to dominant trends in scholarship and changing popular attitudes, Harry Clor presents a careful, reasoned argument on behalf of what he calls "an ethic of decency" (p. 14). This ethic, he suggests, is essential to the health and survival of liberal democracy in ways that are rarely recognized today. He writes for both the educated citizen and the professional scholar, with the earlier chapters addressing broad subjects in nontechnical language and the later chapters focusing on more abstract, scholarly controversies. The argument of the book is characterized by the same moderation that Clor hopes will reemerge in public affairs. Its style reflects its substance. The arguments that he challenges are never rejected out of hand; all are given their due as he encourages his readers to avoid the extremes of absolutism, on the one hand, and relativism, on the other (p. 7).

Clor's main argument is that liberal democracy depends on moral character. Although the cultivation of virtue is clearly not the end of liberal democracy, Clor contends that our theoretical and practical founders recognized the need for a public morality supported by law. Liberal democracy depends on moral character in two ways. First, citizens who wish to govern themselves need to be capable of self-control; they must be able to avoid at least "the grosser forms of incivility" (p. 68). Second, liberal democracy depends ultimately on mutual respect among citizens. If we lose the ability to respect one another and see one another only as "objects" or "opportunities for self-gratification," then we will become "unfit for sustained cooperation in the conduct of civic affairs" (p. 68).

Public morality is usually supported by two different rationales: One points to the need in any community for common beliefs, and the other concentrates on the desirability of virtue itself. The first view is offered by authors such as Patrick Devlin, but Clor argues that societies with no shared beliefs can survive. Rather, the question is whether such societies are desirable. Clor offers as an example a hypothetical society in which one-third are devoted monogamists, one-third polygamists, and the rest are promiscuous hedonists (p. 49). It would be difficult for Devlin to prove that such a society is destined to disappear. The deeper question is whether "the civic community per se [has] a legitimate interest in maintaining a preferred way of life" (p. 50). The second view is offered by classical philosophers such as Aristotle and focuses on the desirability of virtue, but Clor makes it clear that the liberal regime cannot promote virtue for its own sake. The virtues that can be promoted legitimately in liberal democracy are those tied directly to its health and survival. He calls this an "intermediate morality" (p. 74), something less than classical virtue, and something more than mere abstention from violence, theft, and fraud.

Is a public morality supported by law part of the liberal tradition? Can one find support for it in Locke or in the writings of our founders? Clor argues that one must distinguish between classical liberalism and "a new and more aggressive liberalism" (p. 31). Classical liberalism in many ways assumes that public morality is needed, whether or not it allows for the promotion of public morality as a matter of principle. As Clor puts it, classical liberalism is more "accommodating." But Clor does not deny that, in a fundamental sense, liberalism must be moderated by principles in many ways not "intrinsic to its theory" (p. 173), principles from preliberal philosophy or religion. Challenged today by ideas that emerged after its birth, liberal democracy may need to turn to ideas that preceded its birth for its survival.

Since liberal democracy depends on a morality that is not produced or sustained spontaneously (p. 133), Clor argues that the law must play a role. The law is the authoritative voice of the political community, and while it is true that the law establishes punishments for certain kinds of behavior, its truly important role is that of educator. "The law teaches by promulgating" (p. 78), stresses Clor. It announces that certain ethical standards are the "norms *of the civic community*" (p. 78). Clor admits along with Rousseau that the power of law is limited to the preservation of morality, not the creation of a new morality. Prudence would dictate, therefore, that some morally undesirable activities are too much a part of our society to eradicate. But the law can still be a powerful teaching tool. It cannot be used to create a virtuous regime, but it can help limit the growth of vices that threaten liberal society.

Central to Clor's project is his attempt to revive the classical notion of politically relevant moral harm (p. 111). Clor challenges the contemporary view that the political community can concern itself only with the physical harm that one person may inflict upon another, a view which leads to the "insistence that the liberal state must be neutral toward ways of life" (p. 32). The political community, he argues, cannot ignore the long-term effects of morally harmful ways of life. The liberal state needs to discourage activities that are "dehumanizing" (p. 22). It needs to support a view of humanness in order to accomplish this, which means that the law cannot be indifferent to what is morally better and morally worse.

The issue of dehumanization is especially important for understanding Clor's critique of pornography. His friendly critique of the new feminist movement opposed to pornography suggests that his "ethic of decency" is more direct and relevant than the feminist "ethic of equality." Contrary to authors like Catherine MacKinnon, Clor argues that pornography does more than assert male dominance (although it often does this); pornography reduces the human to the subhuman, denying the possibility of love or mutual respect. Pornography is more than "discriminatory" (p. 189); it robs civil society of the idea of humanness, an idea upon which it depends. Pornography portrays everyone as an object, and civil life is impossible when this is the dominant idea.

Clor's book should be read by everyone who hopes to think about the issue of public morality. His moderate challenge to immoderation is not designed to prove that his view is absolutely right; rather, he hopes to encourage reasoned debate and inquiry. In the final analysis, it may be that Clor's book will be too reasonable to have the kind of effect that it should. Has debate even in the academy abandoned reason too much to tolerate such a quiet book?

Beyond Orientalism: Essays on Cross-Cultural Encounter. By Fred Dallmayr. Albany: State University of New York Press, 1996. 277p. $19.95.

Norma Claire Moruzzi, *University of Illinois at Chicago*

In the nearly two decades since it was first published, Edward Said's (1979) *Orientalism* has become an interdisciplinary classic, a text that has reshaped scholarly approaches to cross-cultural study in a variety of fields. The book is a focused examination of the relationship between power and knowledge, and Said's argument, that "the Orient" as such is field of inquiry constructed in the West, the intellectual apparatus of a larger project of Western economic, military, and cultural domination, still resonates. This work is one of the key texts in a range of contemporary critiques that place the assumptions and categories of modern knowledge into question, and Said has himself developed the explicitly political aspect of his argument in his extensive writings on the Palestinian question. It offers all those interested in the critique of knowledge a provocation to extend the range of their own intellectual concerns.

Beyond Orientalism is an attempt to take up this challenge. Fred Dallmayr is a noted political theorist who specializes in continental philosophy, and here he connects his own work to Said's critique of Western representations of the nonwestern world. The book, a collection of related essays, studies the links between Asian traditions of philosophy and internal critiques of the rationalist model that have emerged within the Western tradition itself. The first chapter deals with Tzvetan Todorov's (1984) *The Conquest of America,* but the other eight essays all discuss Indian philosophers or writers whose work relates directly to trends in Indian philosophy. These pieces will serve many readers as an admirable introduction to the thought of several relatively lesser known Indian and Japanese philosophers, including Sarvepalli Radhakrishnan, J. L. Mehta, the Indologist Wilhelm Halbfass, and Kitaro Nishida and the Kyoto School of Buddhist philosophy. Dallmayr carefully explicates these writers' philosophic concerns, connections among their texts, and links between their texts and the writings of Western philosophers, such as Heidegger, Gadamer, Derrida, and Habermas, with whom his readers are probably more familiar. For a collection of discretely written essays, the book contains a remarkable coherence, and it indicates a sustained engagement by its author with a particular area of Asian thought.

As successful as it is, however, the book is not really an extension of Said's political and intellectual project. That project involved a rethinking of the terms of the cross-cultural intellectual encounter, not simply an invitation to engage with the foreign as the new. Unfortunately, although Dallmayr is attempting to give fresh consideration to the contributions of Asian philosophers to modern epistemology, he does so within the confines of the same familiar intellectual discourse that gives his book its satisfying coherence: the discourse defined by the figures of Gadamer, Derrida, Habermas, and most especially Heidegger. That most of the Asian thinkers about whom Dallmayr writes themselves accept the definitions of the dominant epistemological discourse only highlights this problem in the essays.

Said's work focused attention on the classificatory patterns that shape all cross-cultural projects, especially those that run East/West, and the structures of power that inform them, from the eighteenth century through the present. Yet, Dallmayr misreads Said, equating Orientalism with a historical period (nineteenth-century colonialism) rather than a set of power relations that continue to need to be interrogated. Since Dallmayr is explicitly premising his book on Said's argument, this misreading creates theoretical and methodological problems, and certain of his classificatory patterns are especially troubling. First among them is the selection of authors, which is not at all to say that they are not worth the consideration Dallmayr gives them. These are significant thinkers, yet it is striking that none of them seem to be scrutinizing the role of power in the construction of knowledge that Said emphasized was so important. Instead, Dallmayr seems to have selected philosophers whose work in fact illustrates one of the patterns of the Orientalist project: the reconciliation of supposedly authentic Eastern ways of being or thinking with supposedly completely distinct Western norms. This impression is only strengthened when Dallmayr approvingly notes Radhakrishnan's 1936 appointment to an Oxford chair as a sign of the end of Orientalism, rather than its insidious confirmation, or describes Wilhelm Halbfass as an Indologist who can be an "honest broker" (p. 116) for cross-cultural encounter. These stylistic details are indicative of the deeper problem with the pattern of the selection of authors, which is the question not just of who is being selected but also of who is being left out.

It is troubling that Dallmayr makes absolutely no mention of Muhammad Iqbal, a Muslim Indian philosopher whose 1920s Heidelberg training and concern with the notion of the self in Islamic thought would seem to have made him an ideal candidate for inclusion. Iqbal's absence accentuates the lack of any Muslim thinkers in Dallmayr's discussions and the extent to which the Indian tradition is being constituted, by the authors Dallmayr does discuss, as an entirely Hindu tradition, with a bit of Buddhism thrown in. Perhaps Dallmayr is interested only in what may be considered to be authentically Indian philosophy, that which has arisen indigenous to the subcontinent. But this kind of classification of cultural authenticity has itself been radically questioned by the Subaltern Studies collective, a group of relatively well-known Indian writers (including Partha Chatterjee and Ranajit Guha) who have been at the forefront of revising the terms of the intellectual encounter between India and the West and about whom Dallmayr also makes no mention. While the philosophers who have captured Dallmayr's interest seem to be engaged in the epistemological classification of Eastern (Hindu or Buddhist) compared to Western (secular) ways of thinking, the Subaltern project has involved the interrogation of the historical categories of caste, race, nation, and religion, a deconstruction of precisely the categories that guaranteed the authenticity of the precolonial Indian past, its practices, and philosophies. Dallmayr's decision to ignore the thinkers whose work broadens and reconfigures the tradition with which he wishes to engage limits his own project. *Beyond Orientalism* is a serious, valuable introduction to the thought of writers who deserve more attention in the West. If it is nonetheless not quite post-Orientalist, then perhaps that is in itself an indication of the perseverance of the Orientalist paradigm and the continuing aptness of Said's original critique.

Please Don't Wish Me a Merry Christmas: A Critical History of the Separation of Church and State. By Stephen M. Feldman. New York and London: New York University Press, 1997. 395p. $29.95.

Marla Brettschneider, *University of New Hampshire*

Stephen M. Feldman presents a detailed critique of the history of the notion and politics of the separation of church and state in the West. His central goal is to disprove fundamental presumptions about church-state relations in the United States; for this he uses the test of the Jewish predicament, tolerance, and antisemitism since the establishment of Christianity through contemporary U.S. Supreme Court cases.

In between the introductory and concluding chapters, which lay out the theoretical terrain, are eight chapters that fall essentially into two sections. The first treats approximately the first 1500 years of the Christian era in Western Europe; the second half explores the context within the history of the United States since the colonial period. Chapters 2–5, roughly 100 pages, take the reader through an historical account of church-state relations from the ancient period through the English revolutions. The critical history focusing on the United States is presented in chapters 6–9. These include two chapters on "The Fruits of the Framing," the first of which covers the nineteenth and early twentieth century, while the second addresses late-twentieth-century developments.

This work provides an important deconstruction of what the author calls the dominant view of church-state separation: self-congratulatory on the part of the nation's religious majority and culturally imperialist toward, what the author refers to as religious outgroups, particularly Jews. Feldman identifies two central themes in the hegemonic understanding of church-state separation: that it is a unique contribution of political thought in the United States and that the idea "stands as a constitutional principle that promotes democracy and equally protects the religious freedom of all Americans, especially religious outgroups, including Jews" (p. 4).

On the first point Feldman makes clear that the origins of the concept lie in the New Testament and early Christian political ascendancy in Rome. Feldman writes that it is St. Augustine who began to develop the idea into a doctrine. This historical discussion was revived and redirected when Aristotle's writings again became accessible in the thirteenth century and were used in the Christian context by Thomas Aquinas. The author charts the changes and developments through the centuries, always in relation to the persecution of Jews as an archetypal Western outgroup. Feldman moves through analyses of canonical Western thinkers, such as Machiavelli, Luther, Calvin, Hobbes, Harrington, and Locke, to set the stage for his argument that the very notion of the separation of church and state used in the United States is itself a Christian creation that, rather than securing diversity, reinscribes Christian hegemony in the United States.

Feldman argues that "in protecting the free exercise of religion . . . the first amendment expressly protected the ability of Americans to be Protestant" (p. 167), and thus "Congress predictably enacted legislation bolstering Protestant hegemony" (p. 168). The dominant understanding of religious freedom since Madison—that a vast array of religious beliefs and practices are protected in the United States—exists because this vast array is virtually all Christian. Most citizens are blind to this fact due to their membership in the majority grouping. As a Jew, Feldman can see through such myths more clearly, although he comments that de Tocqueville, as an outsider, saw this quite clearly as well.

The argument that so-called "democratization did not correlate with egalitarianism" (p. 200) is well supported throughout the text. Feldman demonstrates that, from the perspective of minorities, part of separating church and state meant that churches were free from legal limitations in the course of their harassment and discrimination against non-Christians. Jews, for example, have been faced with a no-win situation built into the structure of the church-state relationship: advocate for the abdication of church-state separation so that they could seek government protection and refuge, but "any governmental succor for religion would almost certainly translate into support for Protestantism" (p. 202) due to the overwhelming majority/minority ratio. Despite some progress over the years, Feldman points out that for each "success" there have been equal numbers of costs, and the universalist presumption of Christianity that affects outgroups as cultural imperialism remains alive and well in the United States.

This book may be helpful to scholars in certain important respects in addition to its interesting overall argument. Political scientists do not usually read Western Christian history as part of their academic studies and may find the discussion an interesting reminder of the role of religion in Western politics and thought. Constitutional legal theorists tend not to work in such an historical framework and thus may find the first half of the text an interesting context for assessment of Supreme Court cases found later in chapter 9. Finally, few works in political science attend to Jewish issues or the central role of antisemitism in the development of Western political assumptions and institutions. In this sense, *Please Don't Wish Me a Merry Christmas* makes a contribution to the field.

There are, however, certain shortcomings in the text. Feldman declares that his "precise method is to present what I call a critical social narrative of the separation of church and state" (p. 6). As a self-identified postmodernist, he states that his understanding of power is "everywhere and in everyone"

(p. 6). In keeping with basic postmodernist theory, Feldman states he has avoided neatly situating power in particular individual or institutional hands as explicitly conscious or intentional, that he will locate the workings of power in the symbolic, the structural, and the interaction between the symbolic and the structural, as well as "buried within layers of tradition and culture" (p. 6). Although he is true to his word on the structure of the book, that it will proceed from a critical history to a synchronic critique, the other aspect of his method emerges as rather less than precise.

Much of the claim to postmodern methodology—locating power in the interstices of symbol, structure, tradition, and culture—is lost in the story told. Feldman reproduces hundreds of years of history through the actions and ideas of particular "great men" in a specifically intentional fashion. Since much of this lengthy presentation is easily accessible to mainstream students of the history of Christianity elsewhere, the author perhaps could have used the opportunity to develop his postmodern analysis in a more genealogical fashion. What is most strikingly absent, therefore, is an analysis of other structural pressures in the history delineated. There is no mention of changing assumptions of gender, which fundamentally affect the power dynamics between church and state, no analysis of economic and material concerns in the European investigation, and very little attention to the racialized dimension of the imperialism Feldman seeks to critique. The otherwise persuasive argument about the centrality of antisemitism in the development of Western church-state relations would have benefited significantly in this respect; as much new scholarship demonstrates, other identity constructions—such as gender, class, sexual orientation, and race—have foundationally structured Christian antisemitism throughout history.

Domesticating Passions: Rousseau, Woman, and Nation. By Nicole Fermon. Hanover and London: Wesleyan University Press, 1997. 240p. $45.00 cloth, $19.95 paper.

Rousseau and the Politics of Ambiguity: Self, Culture, and Society. By Mira Morgenstern. University Park: Pennsylvania State University Press, 1996. 270p. $40.00 cloth, $17.95 paper.

Cheryl Hall, *University of South Florida*

In recent years, many feminist political theorists have begun "rereading" canonical texts in political theory with an eye not only to their masculinist tenor but also to the resources they may offer feminist theories as well. Mira Morgenstern's *Rousseau* and Nicole Fermon's *Domesticating Passions* are examples of this development. Broadly speaking, both books address a similar theme: the political significance for Rousseau of women, the family, and passion/desire/love. In the course of addressing this theme, both authors pay particular attention to Rousseau's novelistic works, insisting that they be treated seriously as components of his political argument. Fresh and suggestive readings of *La Nouvelle Heloise,* in particular, are one happy result.

Mira Morgenstern's stated project is to provide "a feminist reading of Rousseau that unpacks the implications of Rousseau's conclusions in a way that can ultimately prove liberating to women and to all of humanity" (p. xv). She believes contemporary feminists can find Rousseau helpful, despite his many negative claims about women and the historical distance of his writing, because he emphasizes the importance of women's agency, and because he highlights the weaknesses of liberalism's dualistic categories of state/family, public/private, and man/woman. Rousseau, she contends, can help feminists get "beyond the sterile choices presented to us by contemporary political life" (p. 8).

In building her argument, Morgenstern's first step is to maintain that Rousseau's overarching concern is the attainment of "authenticity," particularly in the context of a complex and alienating modern age. Second, she contends that Rousseau's response to this difficult task of becoming more authentic is to counsel acceptance of the ambiguity of modernity. Such acceptance increases the possibilities for discovering partial but nonetheless productive solutions to the paradoxes facing modern subjects. As a result, such acceptance also counters the despair that may attend modern life. According to Morgenstern, "the most revolutionary legacy" of Rousseau's writing consists in the argument that attaining authenticity is always possible, that it "can be achieved in the midst of daily life and need not wait for a cataclysmic revolution to set it off" (p. 8). The third and final component of Morgenstern's argument is that this response of Rousseau provides feminists with helpful material in our own struggles in the modern age, especially with regard to liberalism's public/private split.

Despite its framing focus on feminist issues, Morgenstern's book is valuable primarily in its extremely close and careful exegesis of the tensions Rousseau lays out in writing about the problem of authenticity. It provides helpful analyses of Rousseau's complex arguments on language, pity, imagination, love, politics, and the various relationships among these. Morgenstern's readings of *Emile* and, especially, *La Nouvelle Heloise* are also sophisticated and persuasive. In contrast to authors who take the fact that both Sophie and Julie fail and/or die early as evidence that Rousseau himself has failed at his own task, Morgenstern argues that Rousseau is deliberately illustrating the weaknesses of the supposed "ideal solutions" detailed in these novels. This reading opens up new understandings of the kinds of lessons readers may take from Rousseau's literary works. Rousseau scholars will find much that is thought-provoking in Morgenstern's detailed analyses.

Those especially interested in feminist issues, though, will likely be disappointed. To begin with, only the concluding 15 pages of the book are reserved for developing the contemporary feminist implications of Rousseau's argument. Moreover, until the fourth chapter, "Women, Power, and the Politics of Everyday Life," Morgenstern uses the generic masculine in discussing Rousseau's writing, a choice that is acknowledged but not discussed or justified (p. xv) and that obscures the gendered specificities of his arguments. Finally, there is a substantial amount of recent feminist literature, both on Rousseau and on contemporary feminist issues, of which Morgenstern does not make use. The consequence is that she must struggle to reinvent wheels—and with tools that may not be the most appropriate to the task. When Morgenstern writes that women's stress in trying to balance families and careers, while male partners at most "cooperate," "can best be understood" through Rousseau's comment on the dissonance between appearance and reality (p. 242), the reader is tempted to protest. Rousseau may well have valuable insights to offer feminists, and it is important that we take advantage of whatever resources we can. But he cannot do all the work that Morgenstern wants him to do for us, and, fortunately, we do not need him to.

Morgenstern's insistence on Rousseau's optimism in the face of the many obstacles to achieving authenticity is also unpersuasive in the end. After spending most of the book detailing Rousseau's appreciation of these obstacles, she does not present enough evidence to demonstrate the hopefulness she sees in him. The question is: What is at stake in whether Rousseau remains optimistic? Morgenstern seems to suggest that *because* Rousseau is hopeful he is useful to us; that his perseverance and creativity in response to difficulty is important in inspiring our own. But surely Rousseau's arguments about modernity can be helpful to us even if—or perhaps, as some have suggested, precisely *because*—his own response to it is to see tragedy rather than hope. In any case, the conclusions we draw from his arguments need not be dependent on his own. In trying too hard both to rescue Rousseau *from* pessimism and save Rousseau *for* feminism, Morgenstern weakens what is an otherwise strong and interesting reading of Rousseau.

Whereas for Morgenstern Rousseau's arguments about women, passion, and the family all figure into a "politics of ambiguity," for Nicole Fermon they are part of what may be called a politics of education. Fermon's central argument is that, for Rousseau, "the problem of politics is viewed largely as the process of reform and reeducation" of "man's" desires (p. 5). She frequently describes this process as a transformation of "passions" into "sentiments," by which she means the transformation of self-interested emotions and physical desires into feelings of common interest and solidarity. The key point, according to Fermon, is that in Rousseau's work the family should be the site of, and women the agents responsible for, this transformation. But in order for this to happen, the family also must be transformed. A properly structured household will train its members to feel in common with others, prefiguring the possibilities for recognizing and determining the general will in a polity (p. 153). Reforming the family is thus "an eminently public enterprise" (p. 6).

While Morgenstern gives close readings of Rousseau's texts, Fermon pays valuable attention to the historical context of his work. She rightly emphasizes the political importance of sentiment for Rousseau, reminding us as she does that he sees sentiment and reason as tied rather than opposed. Her final chapter, "The Feminine Body and the Nation-State," is the most intriguing one. Here she suggests that "the citizen-body Rousseau invokes is that of the male infant" who requires sustenance and nurture from the "mother" state (p. 162). Working with this metaphor, Fermon makes a number of interesting points about Rousseau's conception of citizenship and nationhood as well as the role that real women must play in constructing them.

At the same time, Fermon's larger argument would have benefited from closer and more inclusive readings of Rousseau's texts. Most of her conclusions rely on *La Nouvelle Heloise* and its context; as a result, there is much discussion about the role of women acting within families to educate sentiments, but relatively little discussion of arguments Rousseau has made elsewhere for male legislators, tutors, and public institutions and rituals to educate citizens. Nor does Fermon devote much attention to the relationships between these arguments. In contrast to Morgenstern, Fermon reads *La Nouvelle Heloise* as a positive model, despite Julie's death, but she does not support this reading with comparable references to the text. Likewise, while she repeatedly speaks of the emotional transformation that Rousseau advocates as turning "passions" into "sentiments," Fermon never demonstrates that Rousseau himself uses this kind of terminology and therefore conceptualizes the transformation in quite this way.

Fermon's efforts to link her reading of Rousseau to contemporary literature and issues, though laudable, constitute another problem area. Partly this is a consequence of poor organization: Brief discussions of contemporary issues are sprinkled throughout the book without clear connections to one another or to the rest of the argument. At times it is not even clear whether and when Fermon has departed from exposition of Rousseau's perspective to make a claim of her own. Indeed, a coherent statement of what arguments Fermon would take from Rousseau, and how she would make use of them, is precisely what is missing in the end. In her last chapter she writes approvingly of "the construction of female sexuality as a maternal field of power that organizes on behalf of the state" (p. 178); yet, she has also demonstrated that for Rousseau this depends on the "domestication" not just of passions but also of women (p. 96). What does Fermon make of this tension? How, in general, are we to weave our way through the various insights and problematic arguments left to us by Rousseau? Unfortunately, Fermon does not offer the reader any help with these questions. Like Morgenstern, she also does not take account of the most recent feminist scholarship on Rousseau. As a result, her book contributes considerably less than it might to the development of this scholarship.

Setting these shortcomings aside, however, both books provide suggestive readings of Rousseau, particularly for readers interested in thinking about the political significance of those qualities, people, and institutions often confined to the "private" sphere in political theory.

Machiavelli's Virtue. By Harvey C. Mansfield. Chicago and London: University of Chicago Press, 1996. 371p. $29.95.

Timothy Fuller, *Colorado College*

Harvey Mansfield brings together essays on Machiavelli he has written over 30 years and adds a substantial title essay. This collection includes Mansfield's introductions to works he has translated as well as essays on Burke, the idea of progress, Leo Strauss on Machiavelli, and the idea of the state. As a whole, it shows the reader how Mansfield's thoughts on Machiavelli have developed and matured over time. Of greatest interest is the 52-page title essay, which sums up Mansfield's thinking as it now stands. It is the most powerful essay in the volume.

Mansfield is among the foremost contemporary Machiavelli scholars and also the most provocative and controversial. First, he is an avowed admirer of Leo Strauss's views on Machiavelli. Careful readers will see, though, that he does not merely repeat things Strauss said. He is a translator of and commentator on Machiavelli's most important works, an original contributor in a school of philosophical thinking that has many variations.

Second, Mansfield is a critic of modern historical scholarship on Machiavelli in particular and on political philosophy in general. This is not because Mansfield is historically uninformed; on the contrary, it is clear that he has mastered the works of his historically minded colleagues and what has been written about Machiavelli's historical context. He spends time, for example, describing what he thinks Machiavelli's own historical perspective on ancient thought must have been by contrast to ours. Knowing these things, Mansfield remains unconvinced that the historian's point of view should dominate political philosophy. Mansfield writes against the scholarly fashion of his age in his commitment to what he understands the philosophic task to be.

At the same time, he also writes for or to his age, which he cares about deeply. It is reliably reported that when students challenge his normal classroom restraint, seeking to know what he actually thinks, he says: "Locke in the short run, Aristotle in the long run." It is useful to know this because Mansfield is sometimes said to be himself a Machiavellian or to endorse the "evil teachings" with which he has been preoccupied for so long. My impression in reading *Machiavelli's Virtue* is that he is warning the optimists of the liberal tradition that there are ineradicable, and deeply troubling, features of the human condition, and of politics, which they discount at their peril and which they cannot combat naively. Those who insist on innocence are too easily tempted by fatuous progressivism and later by despairing nihilism. The invocation of Locke and Aristotle is surely intended to suggest a middle ground that is realistically human, excessive neither in its hopes nor in its fears. It would be quite consistent with his outlook, then, for Mansfield to engage with, and take seriously, this most provocative and controversial of political thinkers for the sake of something he finds better but less dramatic and thus often lost or inadequately grasped in the modern babble and shuffle.

What, then, is Machiavelli's virtue? It is "political virtue," that is, virtue defined within and by politics. Political virtue consists in knowing how to make use of an art or skill to one's advantage in the world. The proper alternative Mansfield finds especially in Aristotle: virtue as moral excellence, the exact nature of which requires much study and reflection apart from political action. It can make itself manifest in politics—even occasionally elevating politics—but is not defined in and by politics and does not misunderstand itself by believing that by entering it can control and transform politics.

Mansfield's alternative is intentionally not Kantian if we understand Kant to have sought to transform Machiavellian politics into something morally transcendent. Rather, Mansfield sees Aristotle as having anticipated the political realism of Machiavelli without being captured by it. Thus, Mansfield may be said to reject extravagant (and modern) idealism about politics in favor of a sober demand for moral seriousness and an admission that politics can never be the highest thing for human beings; politics is where we show we are not angelic.

For one thing, political action can never be purely straightforward in speech or act. Properly understood, this lesson may be learned from the best ancient philosophers without denying the moral "idealism" that accompanied their analysis. Christianity complicates the problem by preaching the Incarnation as the manifestation of possible perfection for humanity but at the price of introducing an ambiguity of motive and ambivalence of conduct into politics, fostering a misleading moral casuistry. Thus, moral virtue, classical or Christian, but especially the latter, is always dogged by moralism which must lead its adherents astray both as to how the world operates and about the motives of their own actions in the political context. One would like to know how Mansfield would assess in detail the Augustinian outlook, since he expounds only Machiavelli's analysis of Christianity, not what the greatest Christian thinkers say for themselves.

At any rate, the merely good do not understand the necessities of the political life and try to behave as if necessity were either absent or only temporarily there. The truly virtuous are not merely good in this sense; they keep their eye on both true virtue and the actual necessities of their time and place. We cannot ignore or circumvent outcomes even if we do not think, as Machiavelli appeared to think, that they may count for everything. The Kantian response to Machiavelli is a high-minded but inadequate understanding of the modern political problem:

> The ancients indicated the beastliness of man covertly so as not to encourage it, or so as not to give offense: such is man's nature that in politics especially he will and must play the beast. But to say that openly is no longer merely to indicate; it is to recommend. It is to *politicize* a teaching that began as nonpolitical (p. 37, italics in original).

We are familiar with the repeated efforts to use politics to end politics. Those who want to be nonpolitical in politics must always fail.

Machiavelli has been successful in persuading many modern people that Aristotle is no longer relevant, that his is the truer insight, and it has been determinative in eliciting a reactive modern utopianism that essays to prove Machiavelli wrong, so to speak, on his own terms.

But those who live by history must die by history. Mansfield understands Machiavelli respectfully, like a true philosophical opponent: We must face the temporal world of politics squarely. But Mansfield thinks that restricting ourselves entirely to the play of temporal events is to concede in advance more to Machiavelli than we should. To study Machiavelli's virtue—indeed, to study it adequately in a historical sense—prepares the way necessarily to recalling other ideas of virtue and to taking up the philosophical dialogue on virtue in which we must finally decide what virtue means to us. For these reasons Mansfield's persistence in challenging many assumptions of modern thought, and his refusal to make the reception of Machiavelli easy, is unavoidably provocative and also important.

Socrates' Discursive Democracy. Logos and Ergon in Platonic Political Philosophy. By Gerald Mara. Albany: State University of New York Press, 1997. 324p. $21.95.

Chris Rocco, *University of Connecticut*

In Book 1 of Plato's *Republic* one finds as many definitions of justice as there are interlocutors. While everyone present recognizes that part of justice which resonates with his particular character, class and soul, no one, except perhaps Socrates, sees justice from a more inclusive perspective. The same may be said of the *Republic* itself or, more generally, of the Platonic dialogues. We have just about as many interpretations of Plato's philosophy as we have interpreters: A quick assay of Plato scholarship reveals approaches analytic, hermeneutic, esoteric, deconstructive, literal, and literary. Such diverse approaches yield correspondingly diverse Platos: liberal, authoritarian, democratic, modern, postmodern, and communitarian, to name a few. What more could or should be added to such a list? Into this welter of Plato studies comes Gerald Mara's "discursive" interpretation of Plato's dialogues. This is no cause for alarm, because *Socrates' Discursive Democracy* is a truly impressive work that does not so much join these previous interpretations as displace them. This is a book that repays careful reading, not only because of its original and sophisticated interpretive approach to the dialogues (about which more later), but also and mainly because this approach leads Mara to some truly fruitful conclusions. Yet, even this does not exhaust the book's usefulness, for while Mara offers us a stimulating new way of reading that solves a number of longstanding problems in Plato scholarship, he accomplishes this task while bringing Socratic-Platonic insights to bear on current issues in contemporary politics and philosophy.

The main political burden of the book is to reinterpret Socratic philosophy's relationship to democracy, no mean feat given the criticisms of the Athenian democracy in dialogues like the *Gorgias* or the attempt to refound all politics, democratic or otherwise, in the *Republic*. The consequent philosophical burden is to reinterpret Plato's alleged foundationalism (p. 7) so that Socrates may emerge as a democratic philosopher and, ultimately, as a liberal ironist of a different sort (p. 258). This political-philosophical program rests directly on Mara's novel interpretive approach, announced in his subtitle, and delineated in the first chapter of the book. Stated briefly, previous interpretations of Plato have focused too narrowly either on what Socrates says, his rational discourse *(logos),* or on what he does, the dramatic context or action *(ergon),* in the dialogue. The result is either Platonic metaphysics or Socratic irony. Mara finds both these approaches onesided and rejects them in favor of a more inclusive approach (To be fair, though, the interpretation of Plato as a metaphysician, as an enemy of democracy in the narrow sense and of discourse more generally, is the view he wishes to combat). In place of metaphysics *or* irony, Mara argues, "Socratic *logos* can only play the constructive role which I have assigned it if its limitations are thoroughly grasped. These limitations are communicated in part by Socrates' *erga.* At the same time, Socratic *erga* make practical sense only if they are directed and thus to a degree surpassed by Socratic *logoi.* Socrates' *logoi* and *erga* are mutually beneficial, then, by being to some extent in mutual tension" (p. 30).

Thus armed, Mara sets out to effect a rapprochement between Plato's metaphysically based foundationalism and the concrete practice of Athenian democracy. The ambassador for this mission is, of course, Socrates' discursive democracy, where Socratic practice becomes something of a middle term between Plato's antidemocratic foundationalism and the licentious democratic equality of Athens. That rapprochement is worked out across four central pillars of Plato's foundationalism: the claims that virtue is knowledge *(aretè),* the portrait of the best city ruled by philosopher-kings *(polis),* the vision of knowledge as the direct encounter with the ideas *(epistèmè),* and the experience of radically transcendent love *(erôs).* These foundational Socratic speeches must be seen within the overall context of Socratic practice, which is rooted in the human world, in a democratic culture "where escape from fragility and mistakes is recognized as impossible" (p. 240). Viewed in this context, the Socrates that emerges from Mara's deft handling of the interpretive controversies is thus neither the metaphysician of the *Republic* nor the ironist of the *Apology.*

Mara's Socrates is multivalent, less easily categorized than this binary scheme allows. He stands in a complex and at times ambiguous relationship to the Athenian democracy. Socrates is indeed critical of Athens and its democratic excesses, yet that criticism emerges out of a conversation with the democracy itself and, more important, recognizes that Socrates' own critical discourse relies upon Athenian democracy as its ground and starting point. For this reason, Mara characterizes Socratic philosophical practice as "discursive democracy" and Socrates as a "discursive democrat," encapsulating in those somewhat contradictory terms the tension he seeks between the rational discourse *(logos)* of the philosopher and the democratic action *(ergon)* of the citizen. Mara concludes, and rightly I think, that "Socrates' most important contribution to political theory is not his sketching of an aristocratic or philosophical politics which stands as an alternative to democratic disorder, but his discursive democratic treatment of the democracy itself" (p. 147). Though Mara does not frame it in these terms, Socrates' criticism of Athenian democracy is an immanent one, a criticism that relies on the peculiarly Athenian practice, embodied in Greek tragedy and the institution of *euthunai* (giving an account of ones' public service), and supported by the context of *parrhesia* (frank or forthright speech), of reflective criticism. Mara thus joins Martha Nussbaum (*The Fragility of Goodness*, 1986) and J. Peter Euben (*The Tragedy of Political Theory*, 1990) in arguing that Socrates' whole philosophical enterprise, no matter how critical of the Athenian democratic tradition it may appear, owes that tradition a profound debt and would hardly exist without it.

But how successful is that treatment? To what extent is Mara able to reinterpret Socrates as a critic of Athenian democracy who is also a democrat? How well do his readings sustain the tension between Socratic speech and Socratic action, *logos* and *ergon?* I suggest a closer look at the relationship between the knowledge of the philosopher and the opinion of the city and its citizens. This may be *the* central theme of the *Republic,* upon which that dialogue's whole social ontology rests. Certainly, Socrates' attempt at grounding politics in an unshakable metaphysical foundation presents post-Enlightenment thought with a seemingly insurmountable challenge. Mara negotiates this challenge by arguing that Socrates' treatment of reality via the ideas certainly devalues our opinions, politics generally, and democratic politics specifically. The brilliant light of the sun stands as a rebuke to the shadows of the cave. Yet, Socrates situates his ascent to philosophical knowledge within the broader context of the beliefs and opinions of his fellow citizens. Socrates conducts his investigations from within the cultural space of the cave. There remains, then, a tension between a posited clarity of knowledge and the experience of ignorance, a tension that generates, as Mara puts it, "the discovery of philosophy" (p. 242).

While Mara engages an impressive range of contemporary theoretical and political positions, he is centrally concerned with Richard Rorty's criticism of philosophical foundationalism and his articulation of a liberal ironist self as the best for which we can hope in a post-Kantian and post-Lockean liberal world. If I get Mara right on this point, then he is quite sympathetic to Rorty's antifoundationalism as well as to his ironic turn in regard to liberalism. Yet, Mara's Socrates does not become one of Rorty's liberal ironists and for good reason. As Mara sees it, Rorty's liberal irony, completely embedded in the local contingencies of liberal culture and institutions, relinquishes any ability to question liberal priorities. Such questions challenge liberal culture's very identity and are thus excluded as inappropriate in a contingent liberal culture which "exercises inclusive control over all potential forms of discourse within its boundaries." Plato's Socrates is a "liberal ironist of a very different sort" (p. 258). Socratic liberal ironism fundamentally challenges the views and practices of the democracy from a position outside them, but at the same time it relies on democratic institutions and culture as enabling conditions. Mara's Socrates stops short of Rorty's liberal ironist by retaining that element of rational utopianism which gives his discursive democratic practice its critical edge. This Socrates mediates between the architectonic metaphysics of Plato and the deconstructive play of postmodernism, resting at neither pole, yet always prompting democratic citizens to reflect thoughtfully upon the shortcomings of their own practices.

As should be clear, this is an ambitious project, one that combines knowledge of a broad range of Plato's dialogues, a sophisticated interpretive approach, and an engagement with an extraordinary number of contemporary thinkers (Habermas, Rawls, Rorty, Barber, Sandel, and MacIntyre, to name a few). Mara relocates Socrates within the Athenian democratic political context from which he emerged and persuasively argues that Socratic practice is superior to the various brands of liberalism, communitarianism, and postmodernism available today. This is a complex, nuanced, and original interpretation of Plato's Socrates, one that ought to be read not only by students of classical political philosophy but also by anyone interested in the current range of debates in contemporary theory. Mara has something valuable to say in all these areas.

The virtue of a work as widely ranging as this one is that it inevitably raises questions for further thought. One intriguing issue that deserves more attention is the relationship between Socrates and Plato. Given the sophisticated interpretive apparatus Mara develops, it is surprising that no systematic account of the relationship between author and historical character emerges. This seems an area rich in complexity and nuance, where the familiar arguments of Socrates as mouthpiece or Socrates as fictional character simply will not do. Since Mara has so many suggestive things to say about what Socrates says and does in the dialogue, I imagine he would do justice to this topic as well.

Judging Rights: Lockean Politics and the Limits of Consent.

By Kirstie M. McClure. Ithaca and London: Cornell University Press, 1996. 327p. $35.00.

Melissa A. Butler, *Wabash College*

Isaac Newton practiced alchemy. He also left extensive musings on theological matters. When, as a student, I first learned about these aspects of Newton's life, I was stunned by what was, to me, their apparent incongruity with my image of Newton as the preeminent scientist of the Enlightenment, the man who devised the calculus and made the movement of the universe comprehensible through three simple laws. Naively, I made the error of assuming that because Newton had seemed the model of a modern man in some ways, he would have been so in all ways. I read the future back into the past and, in so doing, distorted the past.

Kirstie McClure (Johns Hopkins) aims to eradicate similar distortions as they affect the way that students of political thought approach the works of Newton's friend, John Locke. In her attention to the task of recovering the historical Locke, McClure follows a path taken by John Dunn (*The Political Thought of John Locke,* 1960), Richard Ashcraft (*Revolutionary Politics and Locke's "Two Treatises of Government,"* 1986), James Tully (*A Discourse on Property,* 1980), and John Marshall (*John Locke: Resistance, Religion and Responsibility,* 1994), among others. Readers will want to compare her emphasis on the central importance of the theological perspective to Locke's outlook with the views of those authors. McClure warns that efforts to categorize Locke as a liberal or as an empirically oriented social scientist simply project nineteenth- and twentieth-century values and developments back into the mindset of a seventeenth-century man. Instead, she argues that Locke's worldview was fashioned very differently from our own, grounded in the cosmological concepts of the "Great Chain of Being" or, as she puts it, "God's architecture of order." *Judging Rights* presents an understanding of Lockean thought that bridges the gaps between apparent contradictions and inconsistencies (e.g., Locke's changing treatment of "indifferent things" and the role of the state in policing religious matters). McClure's Locke offers a theistic but not a scriptural point of view. His works, whether *Tracts or Treatises,* limn out the human position in an hierarchical order of creation. Humans are subject to God's law but also adopt the law to guide their actions (p. 45). They live in "creaturely equality" with others of their species (p. 76) and have a right to use the goods of the earth, those things of subordinate nature, as befits their convenience and preserves their lives. In this world, the question for Locke becomes how human agents "discern the requirements of God's moral rule and, if necessary, distinguish them from the conventional understandings operative in any given society" (p. 45). According to McClure, in posing the question, Locke is a theistic individualist, not a possessive individualist. In answering it, he is a moralist rather than a sociologist.

McClure makes a plausible, if not always fully compelling, case by carefully examining the "theistic resonance" found in Locke's political language. Liberty, equality, right, convenience, justification, and property are all explicated in terms of their theological linkage. Once McClure has traced the roots of the language and established the plausibility of her claim that Locke wrote with a concept of "God's design" firmly in mind, she turns to the practical implications of the design—what is commanded? what is prohibited? how do we know? Political problems, then, are preeminently problems of judgment. The need for authoritative public judgments through political society arose from the changed circumstances consequent to the invention of money. Money obliterated the bounds set on property by natural law (i.e., spoilage). Because the value of money depended on consent, the value of property became detached from its use. As a result, McClure writes, "money compromises human access to the rule of virtue and disrupts natural judgments of the requirements of justice in the original condition" (p. 178). The invention of money made it harder for people to know what was their own and threatened the harmony between individual rights and the good of the whole (p. 211).

Turning for support to the educational writings, McClure depicts a Locke very different from the one C.B. MacPherson envisioned in *The Political Theory of Possessive Individualism* (1962). Instead of gauging rationality by acquisitiveness, McClure's Locke shares the concerns of Gerard Winstanley—concerns that disproportionate desires for worldly goods will lead to a neglect of virtue and the care of one's soul (p. 171). Locke's work, according to McClure, is "far from an unqualified approval of monetarized market economics" or a justification of the commercial society of his day. Rather, it insists upon "the moral necessity of civil law to establish the boundaries of selfpropriety, the limits of private right and convenience, the definition of crime and the determination of punishment" (p. 225). Furthermore, McClure argues that Locke conceptualized the role of political power not just negatively, as a care to protect individual members of the community from injury, but positively as well, in terms of producing material benefits for the community as a whole (p. 268).

Ironically, the result of McClure's work is that by scraping away twentieth-century impositions on the seventeenth-century Locke, she uncovers dimensions of Lockean thought more congenial to contemporary communitarians and strong state theorists than to libertarians.

The Heart of Altruism: Perceptions of a Common Humanity.

By Kristen Renwick Monroe. Princeton, NJ: Princeton University Press, 1996. 283p. $29.95.

Howard Margolis, *University of Chicago*

This valuable and often moving book is divided between accounts of Monroe's extensive interviews with people who have made exceptional commitments to helping others and analysis that (mainly) challenges rational choice analysis of social behavior. In particular, Monroe wants to show that behavior exists which not only cannot be reduced to self-interest but also cannot be accounted for by a utility calculus of any kind, even one allowing for motivation beyond narrow self-interest. She covers a range of cases, but the most important for her, and also the most moving and striking for a reader, are cases of people under Nazi rule who risked their lives to rescue Jews. Indeed, on its face such behavior flagrantly defies explanation in terms of a utility calculus. Like the heroes of Shaw's *Devil's Disciple* or Miller's *Crucible,* Monroe's rescuers are ready to risk death (and, as with Miller's hero, some did suffer death) for reasons they see as outside any kind of calculation: It is just apparent to them what the decent and honorable thing to do is, and they are going to do it. Monroe also reports on her interviews with entrepreneurs who are active in some sort of charitable work, philanthropists who are mainly engaged in using family wealth socially, and people who have risked their life in spur-of-the-moment situations to save another life (heroes), providing a spectrum of social behavior increasingly difficult to fit into a rational choice framework.

There is a paradoxical aspect to her analysis. It challenges especially the mainstream rational choice commitment to analysis of choice in terms of self-interest. Yet, a defender of that usual view could admire and be moved by her rescuers, be grateful for what Monroe has done to document these episodes, and yet feel perfectly at ease with a prior commitment to self-interest as the only motivation of essential interest for social science. For on Monroe's account it is only the most extraordinary cases (the rescuers) that clearly meet her definition of altruistic behavior. So, from the self-interest view, an analyst could allow, and indeed even insist, that such exceptional cases arise but set them aside as precisely so rare and special that, however admirable, they can be treated as unimportant for social theory.

Among lumpers and splitters, Monroe joins the splitters. She sees the entrepreneurs and philanthropists of her sample, and perhaps even the heroes (because their actions are responses to sudden emergencies with no opportunity for thought about whether they really want to take such chances to help others) as much like ordinary behavior and perhaps to be modeled with conventional tools. But the rescuers are her true altruists (they reveal the heart of altruism), and they are different from the rest of us. As a lumper, I would have worked harder to see how far their extraordinary behavior may be seen as the most striking end of a spectrum that includes and perhaps can be made to shed light on more ordinary behavior involving a measure of social motivation, and whether aspects of even the rescuers' choices reflected a measure for self-interest. But it is Monroe's book, summing up an extended research program pursued now for some years, and (in the spirit of her book) it seems appropriate and also accurate to say that she has contributed something of real value to the rest of us.

Requiem for Modern Politics: The Tragedy of the Enlightenment and the Challenge of the New Millennium. By William Ophuls. Boulder, CO: Westview Press, 1997. 320p. $30.00.

John Martin Gillroy, *Bucknell University*

A unique depression overtakes the individual who reads William Ophuls. It is not a superficial depression that can be easily dismissed, but a profound depression as one realizes that beneath the polemic and diatribe there exists an essential argument, with a profound truth. Like a Brecht play or a Shakespeare tragedy, this latest offering by the author of *Ecology and the Politics of Scarcity* (1977, 1992) speaks to an essential truth about the condition of our social, political, and economic world that transcends both his historical and philosophical reductionism as well as his exceedingly narrow definition of the Enlightenment project. He provides little hope or solutions, however, just warnings and alarms.

Basically, Ophuls makes two arguments in this book. First, he contends that the liberal-democratic paradigm which has determined the evolution of our politics, economics, and social life is inherently self-contradictory. This dominant paradigm has produced an antiecological (p. 43), despotic (p. 236), and narcissistic (p. 53) reality that exhibits moral entropy (p. 45) and nihilism (p. 194), while it produces a mass of dead citizens (p. 75) or amoral individuals (p. 27) who are slaves (p. 170) rather than free persons. Second, Ophuls traces this evolution of totalitarianism and despotic democracy, inevitably, to the principles and philosophical assumptions of the Enlightenment. "The Enlightenment Project Has Failed—and failed badly, exposing humanity to an unprecedented planet-wide catastrophe-in-the-making that is the ironic product of its highest ideals" (p. 279).

In the first argument, although Ophuls overreaches in places (making the Marquis de Sade an Enlightenment philosopher [p. 45] and agreeing that literacy/writing is inherently imperialist and tyrannical [p. 179]), he makes some essentially enlightening points about the inherent contradictions in market assumptions about freedom, equality, and private property. For example, his observations about the inherent failings of self-interest as a definition of reason are persuasive, whether applied within an economic context (where means and wealth maximization bury moral ends, positive freedom, and the value of the person) or within a political context (where personal preference and desire satisfaction promote interest and ideology over public spirit).

Throughout this critical argument, which takes up 270 pages of the book, Ophuls exposes the contradictions and inherent problems with the fundamental assumptions and principles that have determined who we are and how we live in modern society. It is here, however, where Ophuls shortchanges himself by ignoring a pertinent source of hope and solutions in his determination to be persuasive and single-minded about the problem and its origins.

In his second argument, Ophuls defines the Enlightenment as a single set of preference utilitarian principles and market assumptions. In this way, he dismisses the greater Enlightenment dialectic, which could be used to provide different principles and assumptions that might effectuate the paradigm shift toward "ecological maturity" (p. 272), a "politics of consciousness" (p. 274), a "recovery of morality" (p. 274), and a "restoration of governance" (p. 276), all of which he maintains we need to navigate into the future. This is the fundamental oversight of the book.

If one studies the Enlightenment and the various actors and arguments that it produced (e.g., see James Schmidt, *What Is Enlightenment?* 1996), then one is on sounder ground in stating that the market-based paradigm Ophuls describes and condemns is but one argument, albeit a dominant one, within an ongoing dialectic that is our true legacy from the Enlightenment. Using that dialectic as a point of departure, and agreeing with Ophuls that "to know the root ideas or governing paradigms of a culture or civilization is to understand its behavior" (p. 178), the task is to find, within Enlightenment discourse, the antithesis and synthesis to Ophuls's thesis, in order to determine the possibilities of a better future.

In *The Lincoln Persuasion* (1993, 50–63), the late David Greenstone describes the essential dialectic within American liberalism between what he calls "humanist liberalism" and "reform liberalism." Humanist liberals, who are concerned with instrumental values and individual preferences, approach government with a utilitarian calculus that parallels what Ophuls argues is the sole legacy of the Enlightenment. Reform liberals are concerned with positive rather than negative freedom and with the moral capacities of individuals and their development rather than with preferences and desire satisfaction. What is most valuable about Greenstone's dialectic, for our task, is its insistence that the Enlightenment project is concerned with a discourse between alternative paradigms seeking core status. If the Enlightenment is a multiheaded creature with competing principles and assumptions about who people are, how they act collectively, and what the proper role of the state is, then the entire project cannot be condemned for the failures of but one of its lines of argument.

Let us suppose that the Enlightenment is not just the evolution of a preference utilitarian market paradigm that originated with Hobbes and evolved through the thought of Locke, Smith, and Madison. In the same way that Greenstone paints a picture of competing paradigms, one essentially Jeffersonian and one beginning with the thought of John Adams, we can argue that another strand of Enlightenment argument begins, for example, with Aquinas and evolves through Rousseau and Kant. This second strand might promote positive freedom and the intrinsic value of humanity and nature over instrumental values and the exploitation of nature. It might focus on essential capacity as the core of practical reason rather than preference and choice. It might be concerned with quality rather than quantity, with organic growth rather than exponential, with ends rather than means, and with politics as a check on avarice rather than a facilitator of desire satisfaction. Perhaps this alternative Enlightenment paradigm, as the core assumptions of a new political and economic order, could reconfigure republican government so that instead of involving itself in the affairs of individuals, it "administers justice among men who conduct their own affairs" (p. 262).

From this viewpoint, social evolution would not be different from natural systems evolution. When charting the development of a system from "pioneer" to "climax" conditions, Ophuls argues (pp. 12–5) that each system has within itself the alternative capacities allowing it to mature and change while maintaining its functional validity. Conceivably, within the dialectic that is the Enlightenment, we have alternative assumptions, principles, and priorities that will allow us to move from "pioneer" to "climax" conditions without destroying ourselves in the process. If this is true, then when Ophuls dismisses the entire Enlightenment project because he condemns one of its products, he ignores principles and historical context that are available for adoption as the core of a new paradigm, one which may take us past the pioneer stage and to the next level of sociopolitical evolution.

At minimum, to complete his critical thesis, Ophuls needs to present an argument that the alternative lines of discourse within Enlightenment philosophy (recognized, for example, by this writer, Schmidt, and Greenstone) are not really alternatives at all. Yet, if Ophuls had separated his critical argument about the market paradigm from his polemic about the Enlightenment and made the search for constructive alternatives his task, then the book might have been a profoundly more positive and invigorating contribution to the literature.

If instead of a requiem Ophuls had written a "Fanfare for 21st Century Politics," then the human energy expended on one's Ophulian depression could have been better used in both acknowledging the failures of the past and planning a new course toward greater success in the future.

The Legacy of Rousseau. Edited by Clifford Orwin and Nathan Tarcov. Chicago: University of Chicago Press, 1997. 352p. $17.95.

Eve Grace, *Colorado College*

The premise of this fruitful volume is that we can better understand modernity's discontents by returning to one of the first radical antimoderns: Rousseau. The editors asked the contributors of these fourteen essays to consider, from our vantage point at the end of the twentieth century, how current perceptions of various issues that face us derive from or are challenged by Rousseau's multifaceted and sophisticated diagnoses of modernity (pp. xi, xii). Contemporary political theory continues to provide evidence that Rousseau's influence on our theoretical discourse is alive and well; his criticism of bourgeois society as a false promise of peace, freedom, and equality whose only result will be a covert state of war characterized by inauthenticity, lack of freedom, and inequality continues to reverberate. How Rousseau's influence has historically operated to shape our current institutions, laws, moral and political habits, and hence our "current selves" (p. 274), however, is not really the purpose of this book. The aim of this rich and stimulating collection is primarily to investigate the reasoning of a thinker who continues to illuminate our theoretical and moral assumptions and to evaluate the ambiguous possibilities of reform or causes for pessimism to be found in his thought.

The essays range widely, discussing not only Rousseau but also a variety of his successors as well as contemporary thinkers, relating "Rousseauian-ism" (both narrowly and broadly conceived) to contemporary experience or questions. The result is a diverse collection of complex, carefully crafted, and thoughtful essays, of which only the principal themes can be adumbrated here.

The volume begins with an essay by Werner Dannhauser comparing Rousseau's critique of the bourgeois to that of Marx and Nietzsche, followed by Christopher Kelly's careful brief rendition of Rousseau's complicated position on the arts as a source of both political corruption and cure, and on philosophy and art as forms of expression. Four subsequent essays explore the contributions of Rousseau to the development of the problems of nature, culture, and history. Susan Shell argues that the idea of history arises from Kant's assessment of Rousseau as the Newton of the moral world. Richard Velkley, in a wide-ranging but dense and provocative essay, traces the genealogies of current critiques of universalist and rationalist thought, and of the complex, tension-ridden concepts of "culture," beauty, and nobility, from Rousseau through his German successors. Joel Schwartz compares Rousseau and Freud on the nature of human sexuality and the psyche. Roger Masters discusses the less well-known scientific legacy of Rousseau's work and engages the essential question of the accuracy of Rousseau's claim to a scientific understanding of human nature in light of recent findings in ethology, neuroscience, and anthropology.

The more directly political section of the book features essays by Marc Plattner on Rousseau as the founder of modern nationalism, Allan Bloom on Rousseau's critique of liberal constitutionalism (reprinted from his *Confronting the Constitution,* 1990), and François Furet on whether the French Revolution actualized the *Social Contract.* In one of the most interesting essays, Pierre Hassner probes Rousseau's rarely discussed conception of international relations as a central political problem. Hassner argues that, torn between patriotism and cosmopolitanism, Rousseau offered practical solutions that simply replicated the very contradictions within the political order and within the individual that he wanted to suppress. His troubled legacy is mirrored in two contradictory current preoccupations: national identity and the politics of ethnicity, on the one hand, and globalization and the politics of the new world order, on the other (pp. 204, 210, 216). Through an appraisal of Taylor's "The Politics of Recognition" (in Amy Gutmann, ed., *Multiculturalism and "The Politics of Recognition,"* 1992), H.D. Forbes critiques current understandings of authenticity and the politics of difference in light of Rousseauian political psychology. He argues that Rousseau differs from "liberals like Taylor . . . [and] more 'postmodern' partisans of difference" in that he rejects internal ethnic diversity as incompatible with the "basis of good politics" and denies the authenticity of an identity based on "dialogical relations" (pp. 238–40).

The volume concludes with three essays from Steven Kautz, Arthur Melzer, and Clifford Orwin that examine three contemporary problems or issues: privacy versus community, sincerity, and compassion, respectively. While Kautz offers a critical analysis of "a formidable contemporary Rousseauian, Richard Rorty" (p. 249), Melzer and Orwin provide characteristically meticulous explorations of some aspects of the rich Rousseauian analysis of the self that serve to elucidate the current creeds of sincerity or authenticity and compassion.

The contributors acknowledge the influence of Allan Bloom, a well-known interpreter and translator of Rousseau, on their understanding of Rousseau (pp. ix). According to the editors, the common thread uniting these essays is Rousseau's famous diagnosis of the "bourgeois," namely, ourselves, as the locus of modernity's ailments. As almost every contributor tells us, the bourgeois—other-directed, alienated, weak, and insincere—suffers from division or disunity between inclination and duty, from the problem of "human wholeness." Charges of human hypocrisy and of internal contradiction, however, are much older than Rousseau, even if because of him society is now understood to be the cause. Furthermore, Rousseau himself is quite willing to concede that the bourgeois is not necessarily discontented or pained by his disunity. That is, questions persist regarding "wholeness" as the ground of Rousseau's critique of modernity, though some of the contributors point to Melzer's outstanding endeavor to settle this issue (*The Natural Goodness of Man,* 1990). Greater discussion by the contributors regarding the precise character of "wholeness" or of the purported damage suffered by we modern liberal democrats according to Rousseau, therefore, would have been welcome; in a way, though, this is an obviously unfair request, given the complexity of the issue and the format of the book.

Another further investigation to which this volume points is the legacy of Rousseau's teaching that we are "by nature homeless," that "it is hard to see any longer what guidance our speculations about nature or human nature can provide" (p. 267). The introduction states both that "postmodernists" sometimes claim Rousseau as a fellow traveler and that they may remain "prisoners of the premises" of a modernity whose most fundamental philosophical claims they, unlike Rousseau, tend not radically to reopen or examine (p. xiv). While Rousseau is one of the first to have described the self and morality as historically contingent and socially constructed, he does continually and emphatically point to a "nature," though one which is nevertheless almost entirely inaccessible. (Hence the statement that the self articulated by Rousseau is encapsulated by the phrase "I must 'be myself' regardless of *what* I may be" [p. 252] is surely too strong; an accurate description of Rorty perhaps [p. 256], but not of Rousseau.) A direct confrontation between postmodern thought and Rousseau on this question of the status of nature would cast further light on the problematic premises of modernity and on the continuing viability not only of Rousseau's thought but also of the very enterprise of political philosophy itself.

These comments, however, are not meant to detract from the merits of what is already more than hearty fare. The contributors leave us with a deeper understanding of a thinker who had the questionable effect of exciting discontents for which he did provide diagnoses, but for which he conceived no straightforward or certain cures. This questionable effect, however, continues to provoke thought, for as this book demonstrates, the discontents and diagnoses are still with us.

Tragedy and Enlightenment: Athenian Political Thought and the Dilemmas of Modernity. By Christopher Rocco. Berkeley and Los Angeles: University of California Press, 1997. 228p. $40.00.

Denise Schaeffer, *College of the Holy Cross*

"We are much more Greeks than we care to admit." So begins Rocco's book, a far-ranging treatment that includes *Oedipus Tyrannos,* the *Oresteia,* and Plato's *Gorgias* and *Republic* and concludes with Horkheimer and Adorno. Rocco argues that these texts can be used to illuminate contemporary debates about the (im)possibility and (un)desirability of "enlightenment," specifically the debate between Habermas and Foucault. He argues that Habermas and Foucault present overly polarized, falsely dichotomous choices, and he seeks a middle ground that balances both alternatives. "We are left with the unsatisfying dilemma of having to choose between a homogenizing democratic consensus and a perpetually agonistic politics of resistance. Is it possible, however, to envision a democratic sensibility that balances the quest to fulfill the democratic aspirations of critical theory with an attendant politics of resistance meant to disturb and otherwise unsettle the normalizing effects of a democratic order" (p. 137)?

The central four chapters have the same format, providing a textual interpretation and then relating the text to the Habermas/Foucault debate. The interpretations include many thought-provoking observations. For example, Rocco detects that classical tragedies are self-reflective about appropriating the past. In appropriating the traditional myths of the city, and even through the traditional ritual of public performances, the tragedians conservatively affirm the existing social order. But by portraying situations in which social order breaks down, they simultaneously reveal the problems of social order and call into question "the most solid structures of meaning, the most carefully guarded boundaries, and the most piously observed limits" (p. 58). Rocco pays close attention to form as well as content in highlighting important tensions in the texts.

When Rocco links his readings of the classical works to the contemporary debate, however, problems arise. While he claims he is staking out a complex middle ground in this debate—and makes a compelling case for the necessity of such a middle ground—he consistently draws a much sharper distinction between the ancients and Habermas than he does between the ancients and Foucault, and in the end he collapses the ancient and the postmodern. Sophocles' warning "parallels Foucault's ironic observation" (p. 62) that liberation contains and conceals its opposite, and Sophocles' play, "like one of Foucault's genealogies" (p. 63), alerts us to the unexpected and enigmatic. The *Oresteia* reveals that the stable foundations of law and justice are founded on women's exclusion and therefore implies a genealogical critique of democracy. The *Gorgias* reveals that we cannot disentangle power from knowledge—Foucault's most central claim. Rocco does not in fact show us how we are more Greeks than we admit, but instead he proceeds as though the Greeks are even more postmodern than we are.

The reason this is disappointing is that several specific observations Rocco makes in the course of examining the texts offer as much of a correction to postmodernism as to critical theory, although he seems reluctant to draw our attention to them. For example, Rocco points out that while *Oedipus Tyrannos* reveals the limits of reason and clarity, it also "makes a coherent point about incoherent speech" and demonstrates that reason "discloses aspects of ourselves and of the world in which we live" (p. 62). But then Rocco uses the play alongside Foucault to criticize Habermas only. He therefore misses an important opportunity to place the ancients and postmoderns in a truly two-sided dialogue, each side contributing something distinctive. When he later criticizes Foucault directly it is because he "refuses to inquire into the motivations that guide his own critical activity" and therefore courts the "danger of becoming a new norm, standard, or center, which would then require its own genealogical critique" (p. 96). To Rocco, it seems, Foucault is not genealogical *enough.* Socrates is held up as the hero on that score, a Socrates who emerges not from the text of the *Republic* but from the "countertext," which allows us to see past "the shortcomings of the Platonic theoretical imagination" (p. 103). Rocco criticizes Plato for betraying the intention of Socratic philosophy because the *Republic* "entertains the idea that objective knowledge of the Good is possible and perhaps necessary for establishing the well-ordered city and soul" (p. 128). This is puzzling. Not only is it unclear why Rocco would contend that Plato does and Socrates does not entertain such thoughts, since Plato only expresses such thoughts in Socrates' voice, but also and more important, we must ask how a *refusal even to entertain the idea* that knowledge of the Good is possible or necessary can be taken to illustrate openness.

We are told several times that the project is aimed at moderating genealogy as well as critical theory, but the goal is characterized as the achievement of "a wholly ironic stance toward the standards of enlightened reason" (p. 36), and one cannot help but suspect that antirationalism is dictating to reason and not the other way around. The few times Rocco ventures appreciation of the positive side of enlightenment, it is in the form of brief and vague generalizations, such as the observation mentioned above, that reason discloses aspects of the world. Which aspects of the world? How? Such statements are not developed and can get lost in the pages and pages of evidence that reason is tyrannical and enlightenment illusory.

Part of the problem lies in Rocco's slippery use of the concept of "enlightenment." Early on in the book it is used rather broadly, associated with illumination, clarity, and progress from deception to knowledge. What seems to be at stake is the very possibility of knowledge. But as the book progresses, the target seems more and more to be "the Enlightenment" and its specific claims. Modernity, science, reason, communication, progress, and democracy are treated almost interchangeably. This makes it difficult to know just how far Rocco means to go when he says that "reason and the mastery it brings constitute the obverse side of a savage tyranny" (p. 186).

The final chapter of the book is a reading of Adorno and Horkheimer's *The Dialectic of Enlightenment,* which Rocco presents as a contemporary model of what it means to think two sides of the tension, both sides of the paradox of enlightenment. But again, one side of the tension is downplayed. Rocco is most likely placing greater emphasis on the side of the tension—resistance, subversion, and tragedy—that is too often underemphasized in "modern" thought. But at a time when our confidence in reason has "irreparably fractured and fallen away" (p. 35), the important tension he has identified, like the crucial middle ground he seeks, threatens to collapse into the side he bolsters as much as the side he attacks.

A Conscience as Large as the World: Yves R. Simon versus the Catholic Neoconservatives. By Thomas R. Rourke. Lanham, MD: Rowman and Littlefield, 1997. 287p. $62.50 cloth, $22.95 paper.

Mark David Hall, *East Central University*

The Roman Catholic Church is often viewed as a bulwark against change of any sort. Over the past 35 years, however, Catholic doctrine has evolved rapidly, particularly in the realms of politics and economics. Most significantly, the Second Vatican Council (1961–65) warmly embraced religious liberty and democratic institutions. Observers generally credit American Catholics, especially John Courtney Murray, for leading the church in this new direction.

More recently, American Catholics have encouraged the church to embrace capitalism. Although the church never rejected this economic system, ever since *Rerum Novarum* (1891) many Catholics have been critical of it. In America this is exemplified by the bishops' 1986 pastoral letter *Economic Justice for All.* In 1991 Pope John Paul II issued the encyclical *Centesimus Annus.* Virtually all interested parties agreed that this document contained unprecedented (but not uncritical) support for capitalism. American Catholics were again credited with encouraging the evolution in church doctrine—this time under the leadership of Michael Novak.

Thomas Rourke is troubled by the rise of politically and economically conservative Catholics, often referred to as neoconservatives. His book provides a passionate critique of their work, especially as exemplified by Novak's *Catholic Ethic and the Rise of Capitalism* (1993) and Richard John Neuhaus's *Doing Well and Doing Good: The Challenge to Christian Capitalism* (1992). Although Rourke claims to be examining the thought of Novak, Neuhaus, and George Weigel, the brunt of his attack clearly falls on Novak.

To criticize the Catholic neoconservatives, Rourke relies on the great philosopher Yves R. Simon (1903–61). Born and educated in France, Simon taught at the University of Lille before coming to the United States in 1938. Here he taught at Notre Dame and the University of Chicago until two years before his untimely death. Simon's early works made important contributions to the study of the ontology of knowledge and the nature of freedom and determinism. As a disciple of Jacques Maritain, he was drawn to political and economic issues. World War II seems to have ignited this interest, leading him to write a variety of significant books on these subjects, most notably *Philosophy of Democratic Government* (1951).

The core of Rourke's book is composed of four chapters which contrast the views of Simon and the neoconservatives on practical reason, politics, economics, and culture/morality. In each chapter Rourke outlines the neoconservative positions, explains Simon's approach to the same issues, and then evaluates the two sides. Inevitably, he determining that whenever the neoconservatives depart from Simon, they are wrong.

Rourke admits that there are areas where he, Simon, and the neoconservatives agree. Yet, early in the book (p. xi) he states that he is going to focus on the areas of disagreement. True to his word, Rourke provides a many-sided critique of neoconservative thought. The full range of his arguments cannot be evaluated here, but a few key ones can at least be mentioned.

Generally, Rourke believes that neoconservatives have been too eager to abandon the Catholic social thought tradition in favor of classical liberal institutions and ideas. This leads them to neglect or misconstrue Catholic views of the common good, authority, and freedom (among other things). Ultimately, he fears, this causes neoconservatives to embrace a concept of society and the state that favors capital over labor, individuals over community, and nihilism over morality.

The strongest aspect of Rourke's book is his clear presentation of the essential elements of Simon's political philosophy. Although never forgotten in certain circles, Simon deserves to be much better known by contemporary political scientists. His insights into the nature of practical wisdom, ethics, democracy, freedom, authority, and the common good provide a useful corrective to many contemporary thinkers, especially those in the classical liberal tradition. Because Simon grounds all his arguments in reason and experience, as opposed to the authority of the church, non-Catholics (like myself) can benefit from his work.

Less persuasive are Rourke's criticisms of the Catholic neoconservatives. The fundamental problem is that he often attributes positions to them that they would reject. This is not to say that Rourke is disingenuous. Rather, he seems compelled to interpret the neoconservatives in the worst possible light (from the perspective of Catholic social thought). In his hands, neoconservatives become advocates (albeit sometimes unwitting ones) of immoral corporations, power politics, cultural imperialism, moral nihilism, and so on.

In some cases Rourke's criticisms point to a genuine gap or ambiguity in neoconservative thought. For instance, Novak and others could be more clear regarding the government's positive role in securing the material common good. Generally, however, Rourke's criticisms would have been more convincing if they had been addressed to a more recognizable version of Catholic neoconservativism. (Novak complains that Rourke misconstrues his work in Michael Novak, "A 'Catholic Whig' Replies," *Journal of Politics* Spring 1996, pp. 259-64).

This is not to say that there are no disagreements between Simon and the neoconservatives. Rourke correctly points out that they clearly diverge on certain practical matters, most notably on economic policy. For instance, Simon is sympathetic to traditional Catholic concerns about "speculation," "illusory services," "unjust wages," "unequal exchanges," "wealth leaking out of society," and the like. He is therefore more critical of capitalism and is much more willing to support government regulation of the economy than are the neoconservatives.

To a certain extent, however, the above comparison begs an important question. This is the degree to which Simon would have persevered in his traditional (and somewhat romantic) economic views. Remember, Rourke is criticizing *neo*conservatives. Thirty-five years ago Novak, Neuhaus, and Weigel largely agreed with Simon's economic views. They changed their positions because of their perception that government programs and policies often do more harm than good. Although they are not libertarians, their views of practical reason and subsidiarity led them to conclude that governments should have a minor role, or no role at all, in certain areas, such as family policy and economic regulation.

Rourke obviously thinks that Simon would not have departed from the views he set down in the 1940s and 1950s, but the neoconservatives are less sure. For instance, Novak, who refers to Simon as a "great scholar," uses his work extensively in *Free Persons and the Common Good* (1989). Novak (1989, 222) admits that he emphasizes those aspects of Simon's work that agree with his own positions, but even so there is clearly significant agreement between the two.

Rourke provides an invaluable service by bringing Simon to the attention of contemporary scholars. Although his criticisms of the neoconservatives are not overly persuasive, even here he raises questions that should concern them. It is hoped his book will inspire additional Simon scholarship, not simply by opponents and proponents of Catholic neoconservativism but by anyone interested in truth.

John Dewey and the High Tide of American Liberalism. By Alan Ryan. New York: W. W. Norton, 1995. 414p. $30.00 cloth, $17.00 paper.

Alfonso J. Damico, *University of Florida*

During the first half of the twentieth century, John Dewey came closer than anyone else to setting the terms of debate for discussions about the nature of philosophy, education, and democracy in America. But by the time he died in 1952 at the age of 92, Dewey's reputation was already on the wane, a process that largely continued for the next 30 years and seemed to forecast the end of his influence. There is something remarkable, then, about what Alan Ryan aptly terms Dewey's "resurrection." Much of this can be attributed to the uncommon vigor with which the philosopher Richard Rorty (*Consequences of Pragmatism,* 1982) has pressed Dewey's claim that philosophy is better understood as cultural criticism than as truth's gatekeeper. Similarly, Jurgen Habermas's efforts (e.g., *The Theory of Communicative Action,* 1984, 1987) to model democracy as an instance of communicative rationality has created new interest in Dewey's writings on democracy. Dewey was no phrase maker, but the idea of discursive democracy was clearly foreshadowed by his *Democracy and Education* (1916). But Dewey's philosophy is important for reasons beyond how it anticipates or influences the work of others. What is not surprising, then, is that revival of a Deweyan outlook in philosophy and political theory has led to several new full-scale studies of his life and times.

John Dewey and the High Tide of Liberalism is one such work. Ryan's book will invariably be compared to Robert Westbrook's splendid and comprehensive *John Dewey and Democracy* (1991). Westbrook argues that Dewey should be read as a critic of rather than as a contributor to liberal orthodoxy. In his view, Dewey's strong commitment to participatory democracy put him at odds with a great deal of American liberalism. Ryan, in contrast, tends to view Dewey in a larger transatlantic light, comparing him to various British philosophers of the early twentieth century. This more transatlantic Dewey is a fairly typical liberal with a strong social-democratic stripe. One other difference also becomes immediately apparent. Westbrook has produced a large intellectual biography that, for example, goes into great detail when discussing Dewey's writings. Ryan paints Dewey as part of a much larger landscape. He often has as much to say about why American life provided a hospitable or inhospitable setting for the reception of Dewey's ideas as he does about the technical features of those ideas. And he is very good, indeed, at this. He is an engaging and conversational writer who is uncommonly skillful at characterizing different schools of thought. In reconstructing the conversations and debates between us and Dewey "and between both of us and his contemporaries," Ryan gives us a strong feel for how Dewey himself heeded the pragmatist injunction that philosophy should make a difference in people's everyday lives.

Noting how often Dewey urged both philosophers and ordinary Americans to do better, Ryan points to the "faith" that informs and unifies Dewey's writings. Phrases about the "consolations of faith" or "democracy as gospel" are deployed to reveal something of the temperament with which Dewey exhorted Americans to take up progressive ideas. There is, for example, a nice stretch of discussion in which Ryan compares the role of Christ in the ethics of T. H. Green with the role of religion in Dewey's philosophy. Dewey, working in an American idiom, argued that "institutional forms mattered less than their animating spirit and that 'attitude' rather than dogma, ritual, or authority, was what counted" (p. 101). Ryan puts his finger on something important here. It is important, on the one hand, because, as someone else once said, it is a mistake to criticize Dewey for having institutionalized a mood without recognizing that this was his chief objective and most characteristic achievement. On the other hand, it also says something about why some readers find pragmatism's meaning so elusive. Dewey, for example, endlessly characterized democracy as a way of life, but it is often hard to see exactly what this means. To take just one instance, it is unclear how the manner of someone's private life bears upon the issue of democracy's operation or even its cash value. One could, I suppose, look for help in understanding what Dewey meant by the "Great Community" by trolling for a few maxims in his discussions of evolutionary naturalism, experimentalism, or practical reason. But these ideas can be just as elusive. In all likelihood, Dewey is bound to disappoint readers who approach his writings in the mood and mode favored by either analytical or continental philosophy. Ryan is very good at explaining why this is so. And he suggests that Dewey is better approached and appreciated as a partisan "deeply committed to progress and the expansion of human tastes, needs, and interests . . . and the rule of intelligence" (p. 367). In short, Dewey is better at characterizing cultural progress than at laying out a blueprint for its realization. This may strike some as an odd outcome for a philosophy often thought to be highly practical. But that will not bother Ryan or, he implies, would it particularly bother Dewey, who put his faith in something grander than technique.

Dewey's "faith" was a belief in the essential compatibility of different experiences and the outlooks they engender. Ryan has considerable admiration for the middle-of-the-road course to which this project gives rise. I can only mention here that Dewey's religious background and his lifelong religiousness, albeit unsympathetic to organized religion, play a large role in this discussion. Without endorsing all Dewey's efforts to unify science and ethics, religious faith and faith in reason, problem solving and aestheticism, Ryan believes that he was on the right track. This is to say that Ryan's own cultural and political sensibilities are very much in evidence as he consistently gives the nod to Dewey in some of his more famous quarrels with people such as Robert Hutchins and Walter Lippmann. *John Dewey and the High Tide of American Liberalism* becomes the occasion for a defense of the "grandeur and humanity" of a middle-of-the-road liberalism.

I do not doubt that Dewey made many of the arguments that Ryan here reconstructs in favor of a liberalism neither obsessed with rights nor satisfied with quick policy fixes (pp. 366–7). But Dewey is fortunate to have found someone so adept at translating his "faith" into a vernacular often more accessible and powerful than the original. One less satisfying consequence is that Ryan does not press Dewey as hard as he deserves. He is well aware that Dewey's optimism is often mismatched when faced with the nastiness of political life (e.g., World War I) or that his view of ethics as satisfying activity is irremediably ambiguous. These are among the standard and legitimate complaints about pragmatism. But the complaints have been rehearsed for a long time now. It is far more satisfying to be reminded that Dewey, in seeking to articulate an American public philosophy that would be both grand and practical, nearly pulled it off.

Liberal Modernism and Democratic Individuality. Edited by Austin Sarat and Dana R. Villa. Princeton, NJ: Princeton University Press, 1996. 345p. $59.50 cloth, $18.95 paper.

David Weinstein, *Wake Forest University*

According to Austin Sarat and Dana Villa in their introductory essay to *Liberal Modernism and Democratic Individuality,* George Kateb trusts that the "redemption of modernity hinges upon the possibility of" what he calls "democratic individuality" (p. 12). Democratic individuality is not just another vain round of contemptible, liberal bad faith. Rather, it ostensibly transforms the Kantian imperative against using people as means into a "fundamental background assumption of everyone" while promoting, nevertheless, a venturous "way of being-in-the-world that is (potentially) free of all conventional limits" (p. 14). Democratic individuality, in short, heroically and inclusively poeticizes the world.

These are not modest claims. And Kateb's Emersonian liberalism is indeed ambitious. It aims to offer a palpably distinct and heartening alternative to the alleged, suffocating perils of communitarianism, all the while avoiding the equally suffocating conformism of market liberalism. Democratic individuality is a rights-based, negative freedom liberalism peculiar to American civic culture that valorizes self-reliance, authenticity, self-experimentation, and diversity. Democratic individuality is a form of moral aestheticism that wonders at the world, at its human difference. In cherishing difference, democratic individuality is also quick to defend it, to join battle against whatever threatens to homogenize it. For democratic individualists, difference is unthreatening. But it is not the symbiotic, unity-in-difference dreamed of by nineteenth-century new liberals and twentieth-century Rawlsians alike. "Rather, the concurrence of developed individualities would be more like a riot of colors, a field of wild flowers, or the spangled garment of uncertain taste to which Plato likened democratic culture." Democratic individuality is not the "good life" of some speciously harmonious whole, though it is nevertheless "sweet," for it elicits "lightness of being," and "free being is light." (George Kateb, "Democratic Individuality and the Meaning of Rights." In *Liberalism and the Moral Life,* ed. Nancy L. Rosenblum, 1989).

Liberal Modernism and Democratic Individuality is an eclectic tribute to Katebian liberalism by friends, former students, and admirers. The collection, some of whose essays have appeared previously, begins with an ode to Kateb by John Hollander, followed by Cornell West's gentle questioning of the relevance of Kateb's Emersonian liberalism to the mass culture of modern capitalism. Part II pursues the issue of relevance with respect to the intensification of modern identity politics. The essays in Part III are inspired by Kateb's enthusiasm for exploring the ways politics and culture inform each other. Finally, Part IV considers the implications of democratic individuality for civic engagement, returning full circle to the question of the former's relevancy. For instance, Sarat's concluding essay on how capital punishment in the United States displays the democratic state's secret compulsion to assert absolute sovereignty over its citizens splendidly exemplifies Kateb's enmity toward the ever-present specter of statism, showing us how the theory of democratic individuality is, indeed, therapeutically relevant.

Other essays, though, are less generous on the question of relevancy. The late Judith Shklar's contribution, "Can We Be American Scholars?" is ostensibly about the adequacy of Emersonian liberalism formulated in Emerson's 1837 Harvard address, "The American Scholar." Shklar never mentions Kateb, so it is unclear whether he is also her target. But when she accuses Emerson of never being at his "best when he talked of practical reform" (p. 74), we may take her to be aiming indirectly at Kateb through the mirror of Emerson. Not unlike West's assessment of democratic individuality, she finds Emersonian liberalism inspiring yet impractically sentimental and disengaged. Practical relevance seems to be the predominant critical thread that weaves most of this collection's offerings together.

William Connolly worries that Katebian liberalism may be all too relevant by inadvertently fostering evil. Though Connolly readily agrees with Kateb that basic rights are critical for combating evil, he also suggests that Kateb ill appreciates how the politics of rights can accentuate differences needlessly and accommodate violence against those (such as homosexuals) supposedly abnormal. Wherever futile and all-too-common aspirations for "wholeness" infect identity, mere difference is quickly transformed into threatening otherness. Differences are tolerable (after all, identity depends upon them), whereas otherness requires restorative treatment at best and radical surgery at worst. Unfortunately, any democratic culture devoid of vigorous political contestation, though well schooled in the discourse of rights, risks becoming preoccupied with scapegoats and nasty therapies.

Other contributors, like Benjamin Barber (democratic constitutionalism), Amy Gutmann (deliberative democracy), and Leslie Paul Thiele (postmodern environmentalism), provide complementary strategies for immunizing democratic individuality against the excesses of exclusionary otherness. Thiele, for instance, invokes postmodern environmentalism as a way of celebrating and safeguarding robust difference so prized by both Kateb and Connolly. In Heideggerian fashion, environmentalism "discloses" by seeking to "bear witness" to the world in all its immeasurable diversity. Environmentalists "resist the seduction" of trying to "possess and master, extolling the beauty of that which remains other and the wisdom of letting it remain so" (p. 168). Human freedom is now *not* to be sought in mastering the world but through "self-conscious living within limits" (p. 161). But, as Isaiah Berlin has perceptively admonished, retreating to inner citadels in the name of freedom is not necessarily freedom. Asceticism is too often just quiescence. And for Berlin, contrary to Thiele, strategic retreats are invariably exercises in self-mastery anyway!

Liberal Modernism and Democratic Individuality possesses many virtues. It testifies admirably to Kateb's growing legacy. It highlights Kateb's disquietude about communitarianism for supposedly encouraging docility and even barbarism. Sarat and Villa's collection also suggests, in friendly and respectful ways, that we should be no less anxious about the potential hazards of democratic individuality. Its many virtues notwithstanding, both as a tribute to and as a critical assessment of George Kateb's writings, *Liberal Modernism and Democratic Individuality* has its faults. Those who still prefer their theorizing lean and analytical will likely respond to some of the essays as Judith Shklar writes of Emerson in her contribution to the collection: "In my view, miasma is the right word for this sort of talk, and it is also intellectual cowardice to resort to this sort of cover for an otherwise exemplary skepticism" (p. 69).

Democracy's Place. By Ian Shapiro. Ithaca and London: Cornell University Press, 1996. 269p. $45.00 cloth, $16.95 paper.

Henry S. Richardson, *Georgetown University*

This collection of previously published essays erects a multitiered scaffold in preparation for constructing an ambitious normative theory of democracy. As platforms offering critical purchase, the essays represent models of penetrating analysis and assertive synthesis. Insofar as they hint at the shape of a theoretical edifice to come, however, they indicate that the builder will have a difficult task: The criss-crossing levels of scaffold rule out most common ways of building democratic theory. This is no accident: Shapiro deliberately sets out his view "in contrast to liberal, socialist, conservative, and communitarian views" (p. 221). He writes against the social contract theories of Rawls, Nozick, and Dworkin; against the "public choice" or efficiency-based views of Buchanan and Tullock, Riker, and others; against the neo-Schumpeterian, interest-based views of Przeworski, Di Palma, Horowitz, and Huntington; and against Lijphart's consociationist approach. With so much to be against, it would take a bold stroke of theoretical innovation to find a new and acceptable theoretical home. Shapiro's attempt to preview his new view is hampered, however, by a fashionable disavowal of abstract theorizing.

Taken individually, the critical essays in this volume are well worth reading. They exhibit an impressive range and versatility. While Shapiro's essay critical of social contract theories presents little in the way of novel argument, it neatly organizes its complaints around the connection between justice and individual workmanship. Breaking newer ground, the essay attacking public choice theorists elaborates the central insight that there is no reason to grant realism, let alone normative privilege, to a hypothesized state of society absent government "intervention." Here, Shapiro's arguments uncovering false presuppositions are trenchant, forceful, and reinforced by concrete examples from U.S. constitutional law. Neo-Schumpeterians and consociationists are attacked in a more empirical mode, with special reference to the recent transition of power in South Africa. This case study (coauthored by Courtney Jung) generates interesting empirical hypotheses about how the dynamics of negotiated transitions of power affect the health of the ensuing democracy.

From Shapiro's criticisms and from a pair of programmatic essays, some of the main features of his plan for a theory of democracy do begin to emerge. Normative political theory in general, and democratic theory in particular, he holds, have to do with the proper handling of power relations. These, it turns out, are everywhere. Not limited—as liberals tend to suppose—to interactions between individual and state, they also occur pervasively in civil society: in the workplace, in glee clubs, and in the family. Putting Kenneth Arrow's attack on all-purpose social choice functions together with Michael Walzer's idea of different spheres of society, each with its own appropriate principles and institutions of social choice, Shapiro argues for a "semi-contextualized" approach in which democracy calls for different decision procedures in different spheres. Labeling his approach "democratic justice," Shapiro repeatedly emphasizes that while democracy is of some value in its own right, it is not the ultimate criterion of political justice. Instead, it must take its place as an element of a broader approach to the justice of power relations.

One of Shapiro's central themes is that to require unanimity or consensus is to privilege the status quo. He argues that a thriving opposition is an essential element of a just democracy. Against the public choice theorists, this complaint is well taken, and this theoretical positioning is attractive. Against the appeal to consensus by social contract theorists such as Rawls, the argument overlooks the degree of dissension allowed within an overlapping consensus. Shapiro's emphasis on continuing dissent qualifies his incorporation of Walzer. The principles appropriate to the various spheres cannot be derived from extant "social meanings," as these are subject to constant conflict.

Shapiro's attention to multiple spheres draws him toward the civic republican camp. The chapter critical of the Supreme Court's decision in *Wisconsin* v. *Yoder* (written with Richard Arneson) argues that the power of Amish parents over their children does not rightly override the children's right to an education in which critical thinking abilities would be fostered. These abilities are particularly important, Shapiro notes, in nurturing effectively oppositional democratic citizens. While this position is not cozily communitarian, it does place emphasis on what Michael Sandel has called the "formative project" of educating citizens. Of course, if politics is as pervasive as Shapiro suggests, citizens are no more important to democratic theory, in the first instance, than family members or choristers are. We may speculate, however, that Shapiro singles out the project of creating citizens because political measures are necessary for checking power in any sphere.

We must speculate about this, for in this series of preparatory exercises Shapiro is neither explicit about the definition of power nor particularly forthcoming about its sociology. With respect to his observation that the recognition of marital rape was a watershed in the balance of spousal power, for instance, we are left guessing whether that power is to be understood as having had a physical basis (in the male's musculature) or a legal one (in the earlier lack of legal recourse). Shapiro associates power with hierarchies—which should be "presumed suspect" (p. 238)—but does not pause to define those, either. As part of his qualified endorsement of Walzer, Shapiro emphasizes the multiplicity and incommensurability of social goods, which block simple comparisons of more and less. It is surprising, then, that he seems simply to presume that one may always speak of power, despite its presence in every sphere of social life, in terms simply of the more and the less, the "underdog" (p. 51) and the dominant.

That such important points remain unclarified is perhaps accounted for by Shapiro's facile antitheoretical stance. He attacks abstract theorizing as idealistic and as insufficiently contextual, stating by contrast his own affinity with Dewey's pragmatism. While Dewey's observations about the interdependence of end and means in political deliberation deserve our attention, his antitheoretical rhetoric was immoderate, belying his own engagement with metaphysics and too often constructing its "theoretical" opponents out of old shirts and straw. Besides, it is clear that Shapiro is engaged, by means of these essays, in building a normative theory of democracy, one which revolves around an unusually abstract (if still too vague) definition of power. If he is really to make good on his aim of developing a view neither liberal nor communitarian, socialist nor conservative, and is not just going to give us an eclectic theory built of bits and pieces of each, then he will have to continue and deepen the highly abstract exercise of conceptual positioning and integration begun here. Like the shimmering, fabric-sheathed scaffold with which Christo covered the Reichstag, these polished studies make us curious to see the edifice that will emerge from under the wraps and eager to see it sharply defined. Shapiro's essays pique our interest in the democratic theory he is constructing. We ought to be interested in it even if we are skeptical that it will be as novel as it promises to be.

Liberal Economics and Democracy: Keynes, Galbraith, Thurow, and Reich. By Conrad P. Waligorski. Lawrence: University Press of Kansas, 1997. 258p. $29.95.

Claudio J. Katz, *Loyola University Chicago*

In the early 1970s, when even Milton Friedman proclaimed that "we are all Keynesians now," one would scarcely have anticipated that laissez-faire principles would soon dominate public debate. Waligorski's book offers a timely reconstruction of the normative economics of liberal reform. Its argument explores two contested theoretical terrains using as its foil laissez-faire liberalism, derived from Marshall via Hayek and Friedman. (Little attempt is made to address the critique of capitalism of the democratic left.) One terrain is liberal theory itself: Although laissez-faire and reform liberalism share certain ideals (growth, the rule of law, liberty), they diverge sharply in their commitment to others (democracy, equality, participatory autonomy). The second terrain is the economy: Although they share an understanding of capitalism's indispensable features (private property and the market), laissez-faire and reform liberalism provide alternative conceptions of the economy and hence contrasting visions of what capitalism, properly constituted, could become.

Waligorski undertakes to make explicit the underlying moral commitments of major representatives within the reform tradition. One chapter is devoted to its origins at the turn of the century: Dewey, Brandeis, and Hobhouse represented a departure within liberal theory, rooted in a concern that changes in the capitalist economy imperiled liberal norms. Subsequent chapters examine the moral economy of theorists who epitomize contemporary reform liberalism—Keynes, Galbraith, Thurow, and Reich. All are primarily economists who wrote "extensively and for the public about policy and politics as well as economics" (p. x). Waligorski concentrates on their shared "values, beliefs, and policy prescriptions" (p. ix), underplaying some of the critical divisions among them, especially in their views of the fruitfulness of neoclassical economics. This is unfortunate because some of the more interesting aspects of their values and prescriptions reflect differences in their models of the capitalist economy.

The organizing principle of liberal theory—the distinction between public and private spheres of activity—was originally intended to limit government's reach. The capitalist market played a special role in curbing political rule: It professed to dissolve power. This claim could no longer be sustained by the end of the nineteenth century. The epigraph for reform liberalism was written by J. S. Mill: "No longer enslaved or made dependent by force of law, the great majority are so by force of poverty; they are still chained to a place, to an occupation, and to conformity with the will of an employer, and debarred by the accident of birth from both the enjoyments, and from the mental and moral advantages, which others inherit without exertion and independently of desert" ("Chapters on Socialism," in *Collected Works,* vol. 5, p. 710, 1967.)

The conceptual innovations introduced by liberal theorists are well known. The distinction between formal and substantive rights reflects the conviction that meaningful liberty requires both the equal protection of law and access to a share of society's resources. Individual autonomy was broadened to include collective self-government, the right to participate in making the rules to which individuals must submit and those decisions whose consequences they must bear. Waligorski tells this story well; but it is, after all, a familiar story.

More promising is his account of the profound shift in the liberal conception of the market and private property. Two theses, Waligorski shows, distinguish the reform tradition. First, reform liberals reject the laissez-faire premise that the market tends "naturally," that is, in the absence of the distortions introduced by governments, toward equilibrium and harmony. In their view, the capitalist system is inherently unstable, as market networks are easily disrupted by recurring mismatches between supply and demand among the various factors of production. The pivotal figure here was Keynes, who provided the economic analysis that justified political intervention. His central concern was the pervasive unemployment bedeviling capitalist economies. Keynes's diagnosis was as revolutionary as his prescription: The first cause of joblessness lies in capitalism's inability to maintain the requisite level of aggregate demand, and the first cure lies in using government to restore demand to full employment levels.

Second, reform liberals reject the conviction that private property necessarily protects individual liberty. This view was a product of the seventeenth and eighteenth centuries, when economic establishments were small, and large numbers of producers controlled their means of production. Reform liberals contend that the emergence of oligopolistic firms limits the explanatory purchase of laissez-faire economics. Indeed, the myth of a free market is today largely an ideological construct, serving to cloak the realities of power. Explicitly political concepts must be adopted to explain contemporary economic relations. Large enterprises—forms of " 'industrial absolutism' " (p. 25), in Brandeis's words—belie the claim that capitalism achieves cooperation without coercion. Their private power has subverted fair competition: The influence of investors and corporate executives over economic stability ensures that their interests receive special consideration from government officials; property income forms the basic source of unearned wealth and inequality; and the ability of workers to resist desperate exchanges is weakened as they are presented with a " 'choice between starvation and submission' " (Keynes, on p. 65).

The theoretical analysis of capitalism—the fragility of markets and the private power of capitalists—informs the twin policy objectives of reform liberalism. First, measures were taken to protect ordinary human beings, who count as commodities only in the fictitious world of laissez-faire economics. This goal itself has two aims: income maintenance programs safeguard individuals from the vagaries of nature and the market; and regulation of work relations (work and safety legislation, policies strengthening labor unions, education and job training programs) redress the balance of power between employers and employees. Second, public policy is designed to protect capitalism from itself and from more radical alternatives. Galbraith's claim that "Keynes saved capitalism" (p. 149) characterizes the reform project as a whole. Fiscal and spending measures were undertaken to make the crucial capital investments the market fails to make, from road networks to research and development. Their purpose is to foster growth and improve the international competitiveness of the U.S. economy.

Waligorski's analysis of reform liberalism is clearly sympathetic to its aims. He is not uncritical, however. The limits of reform liberalism are best revealed by asking whether and to what extent its two aims—preserving capitalism and ensuring that human beings are accorded equal concern and respect—are compatible. Reform liberals maintain that there is no inevitable trade-off between substantive liberty and equality, on the one hand, and economic growth, on the other. To the contrary, their policy proposals are frequently presented as spurs to efficiency and productivity. But the practice of liberal reform does a disservice to reform theory. Waligorski underscores the problem: "Quite often reform liberals are incisive and indeed radical in their critique and analysis but not nearly so trenchant in their recommendations . . . . The slight edge of radicalism in Brandeis, Dewey, and Hobhouse is not present in Galbraith, Keynes, Thurow, and Reich, who are willing to preserve more existing social-political relations in the economy" (p. 176). Can reform proposals be seriously contemplated without sharply constraining the guild of capitalists? "Reform liberals," suggests Waligorski (p. 168), "confront just the periphery of property's coercive potential. . . . Given their criticism, these theorists need to attack power with more vigor and at its source." Given his own endorsement of their criticism, Waligorski's analysis curiously pulls up at this point, begging the deeper question: Can liberal theory remain faithful to its core principles and retain its historic association with capitalism? In fact, the question cuts both ways. It may be argued that reform liberals abandon their liberalism by rejecting or endorsing more radical reforms. Neither argument is explored in this book.

International Relations and the Limits of Political Theory. By Howard Williams. New York: St. Martin's Press, 1996. 170p. $45.00.

Steven Forde, *University of North Texas*

Howard Williams presents his book as "a progress report upon my own thinking in political theory" as well as a statement about the current unsettled or transitional state of political theory in general. It takes a certain amount of courage to begin a book this way, for one appears merely ludicrous if the progress of one's own thinking should prove to be of less interest to the wider world than one had supposed. Williams is the author of numerous books on political theory, including international relations theory, so his implicit claim is not to be dismissed out of hand. The present work does make some interesting observations about the subject of political theory and international relations. As an attempt seriously to engage the important issues it raises, however, and in that way to advance current discussions in the field, most scholars will find that it falls short.

The book is organized in two parts. The first deals with "Problems of Political Theory," the second with "Problems of International Political Theory." Most of the chapters in both parts revolve around particular political thinkers (Kant, Nietzsche, Hegel, Habermas, Grotius, Locke); others focus on such themes as "Democracy and Human Freedom" or "Justice in One Country?" For the most part, the chapters read as occasional pieces on their various subjects rather than as parts of any coherent, developing argument. The author explains that most of the chapters were originally composed for other occasions, but even allowing for this, the collection hangs together less than its presentation seems to promise.

Individual chapters could serve as useful introductions to the authors or subjects under consideration, perhaps for classroom use, but much if not most of the material is of too basic a nature to detain scholars. The scholarly apparatus is frequently not substantial enough to indicate how the argument is grounded, sometimes giving the impression that the author is simply proceding by assertion. One unfortunate consequence is that, where the author takes a position on some point of controversy, often not enough is said to indicate why this position rather than another was taken. Scholars who take opposed positions will not be persuaded, and serious debate will not be furthered.

The book's overall thesis is that international politics has become so intertwined with the politics of individual states that some corresponding reworking of political theory is required. In itself, this thesis is not novel, and Williams does not claim that it is. He claims, rather, to be "setting out an agenda that is already with us" (p. 157). Some may find, however, that the way the contemporary dilemma is laid out is overdrawn. I can imagine political scientists and practitioners who would quarrel with the statement that "the majority of politics is now international politics," that international and domestic politics are now "indistinguishable" (p. 141). Yet, these statements are backed up with nothing more than a few perfunctory references to nonstate actors, economic globalization, and related phenomena. As to how political theory should respond to this situation, Williams draws, sometimes imaginatively, on the resources of past and present political theory to suggest avenues. Yet, solutions to our dilemma, if there are any here, are vague and sometimes implausible. It is recommended, for example, that we should first make issues of international distribution paramount and then see what the implication may be at the national level (p. 147). States may balk at this procedure, even if a penetrating philosophical justification were presented for it here, which is not the case. It is similarly proposed that we may work our way to an international code of ethics on something like the Habermasian model of open discourse (p. 163). But Williams has already pointed out that a Habermasian speech community may not be possible even among societies as diverse as eastern and western Germany (pp. 54–5). He also endorses Rawls's call for "liberal" toleration for diverse regime types in international society, including the nonliberal (p. 147), after seeming to approve Kant's notion that spreading liberalism is one key to international concord.

Williams has interesting thoughts on such subjects as the implications of the fall of Communism, the tension between the roles of political philosopher and political advocate in Hegel, and the relevance of the misgivings of Hegel and Kant toward unmediated popular input in government. Many of these provide food for thought, but again they are not generally developed thoroughly enough to provide more than that. Following earlier work of his, Williams suggests that Hobbes's laws of nature may provide a basis for international comity. This may have merit—Charles Beitz (1979. *Political Theory and International Relations*) has made a similar argument—but Hobbes explicitly and vehemently rejects the possibility, due to the international security dilemma. Williams gives no indication in this context (pp. 150–1) of why we should be persuaded by him rather than Hobbes on this. Somewhat more plausibly, Williams argues that Locke's natural law provides a foundation for a peaceful international society. But his discussion of the pacific potential of natural law in Locke rests exclusively on early statements to that effect in the *Second Treatise,* without so much as a nod to later statements to a contrary effect. The conclusion he draws—that since legitimate states abide by natural law internally, "by definition therefore Locke would expect the leaders of civil societies to follow lawful external policies" (p. 91)—is not likely to persuade anyone with a more realist interpretation of Locke.

So what is the current state of Howard Williams's thinking on international relations and political theory? Nothing terribly definite, it seems, but a series of vague and sometimes enticing possibilities. There is nothing wrong with this being the state of one's thinking, of course, though the propriety of issuing a bulletin to that effect may be questioned. For readers who want an overview of some basic ideas in international political theory, this book may be serviceable. Those who want to engage the issues more seriously are best advised to look elsewhere.

American Politics

The Pursuit of Absolute Integrity: How Corruption Control Makes Government Ineffective. By Frank Anechiarico and James B. Jacobs. Chicago: University of Chicago Press, 1996. 292p. $24.95.

Michael Johnston, *Colgate University*

No one much likes corruption, and calls for reform are politically popular. But the cures can be worse than the problem itself; at the very least, reformers ought to be aware of the difficulties they can create as they pursue the impossible goal of totally honest government. Frank Anechiarico and James Jacobs make these points effectively in this book, a timely and very useful addition to the new debate over corruption and reform.

The authors focus primarily upon New York City but link their analysis to federal programs and anticorruption reforms when appropriate. Drawing upon extensive interviews and their experiences studying a variety of agencies, they argue that a powerful "Anti-Corruption Project"—a century in the making and enjoying powerful elite backing—has, in the name of reform, made government inflexible, unresponsive, and less able to implement policy effectively. Honest civil servants must negotiate a thicket of rules and investigative bodies, both in the course of their jobs and at times in their personal lives, while the dishonest can hide behind procedures intended to protect whistleblowers. Businesses seeking contracts with the city find the prequalification process a risky and expensive labyrinth, and they risk being banned from bidding because of minor bureaucratic errors. All this happens in a climate of elite and public opinion that shows little realistic regard for the value of politics and the exercise of influence, for the personal and professional rights of civil servants, or even for what actually constitutes corruption. Worse yet, for all their costs in terms of resources and governmental effectiveness, many reforms do little to reduce real corruption. Indeed, by making policy implementation more rigid and cumbersome, they may increase the incentives to engage in it.

The book begins with a useful historical survey of the development of corruption and reform, charting the evolution of the latter from an emphasis upon virtue to the implementation of comprehensive (and often oppressive) surveillance of public employees and their activities. The focus then shifts to personnel systems, with a particularly sharp analysis of how the idea of conflict of interest has become a virtually open-ended concept, and how restrictions against it can produce counterproductive or even nonsensical results. Two succeeding sections take up the modern investigatory and prosecutory apparatus—such as the city's powerful Department of Investigation—and policies aimed at guaranteeing integrity through bureaucratic processes, such as procurement blacklists and the increasing reach of auditing controls. A final section deals with the broader effects of the anticorruption project, not only upon the operations of government but also upon the field of public administration and the ways we think about corruption. Throughout, the critique is sharp and uncompromising, and it is supported by impressive evidence and case studies.

New York City is not the nation as a whole, and American ideas about corruption often do not travel well to the rest of the world. But the central argument of the book is an important one—particularly now that corruption, or the use of corruption as a political issue, figures so prominently in domestic and international politics. Administrators require a measure of freedom and the power to act if democratically made choices are to be realized through public policy. Civil servants are human beings with families, personal histories, and rights. They are not parts of a machine, and they do not and cannot act in a technocratic vacuum; we are not entitled to deprive them of their privacy and dignity in the name of accountability. Total honesty in politics and administration is impossible—just as in any other aspect of life—and attempts to pursue it have real costs in terms of time and resources. Often, allegations of corruption involve little more than legalistic breaches of procedural requirements of reforms themselves rather than real abuse of public roles and resources. Anechiarico and Jacobs do not argue that corruption is somehow "functional" or a good thing, and they do not categorically oppose attempts to fight it. They do, however, convincingly portray New York City not as a sinkhole of corruption but as a showcase for the excesses of reform. In so doing they help account for the bureaucratic inertia, fragmentation, and waste that have made governing that city so difficult.

At times their critique credits the anticorruption project with more coherence and internal consensus than it really possesses, even in New York City. At times we find claims such as "the anti-corruption project and the reigning forces of bureaucracy are beginning to circle their wagons around [the issue of] provisional employment" (p. 185). While New York City does have powerful good-government organizations, often enjoying the active support of the elite news media, reform coalitions are still more loosely knit and transitory than a term like "anticorruption project" seems to imply. Were this not so, corruption would not be the sort of cyclical concern it usually is. Moreover, some advocates of reform are a good deal more politically sophisticated than others, and there is at times more recognition of the political realities of government among reformers than some of the book's accounts suggest. A related issue, perhaps beyond the scope of this book but still worth some thought, is the role of the public: If strident reform crusades do more harm than good—and the authors are persuasive on this point—then can political leaders learn to raise the corruption issue without pandering, and can the public develop a more realistic view of its own?

But these points should not distract us from the strong critique and argument for political realism developed in this book. If New York City, which is probably the most intensively investigated and reformed unit of government in the country, is not better off after a century of reform, then perhaps it is time to rethink our views on politics, administration, and corruption.

Taking the Initiative: Leadership Agendas in Congress and the "Contract with America." By John B. Bader. Washington, DC: Georgetown University Press, 1996. 304p. $49.95 cloth, $19.95 paper.

Donald C. Baumer, *Smith College*

John Bader's book about congressional leadership is an effort to demonstrate that party leaders have been making choices about policy priorities for many years, and these choices can be linked directly to their strategic goals. Bader goes on to argue that the "Contract with America" is best viewed as a continuation of an established leadership pattern of "taking the initiative," rather than a break with tradition. He is only moderately successful in sustaining these arguments, but the book makes an important contribution to the literature on congressional leadership because of the thoroughness of Bader's research and his comprehensive treatment of the subject.

Bader presents a theoretical framework for understanding the choices leaders make about priorities that emphasizes the importance of strategic goals and issue dimensions. According to him, congressional leaders develop their strategic goals first and then look to the issues on the congressional agenda in an effort to find a match. The characteristics or dimensions of the issues have to be weighed very carefully in order to maximize the likelihood of legislative success. Thus, choices about legislative priorities are central to this model, but these choices are said to involve complex political calculations about the potential linkages between strategic goals and issues that are defined by a variety of political characteristics.

Bader brings specificity to this model by offering a list of six strategic goals pursued by majority party leaders and five issue dimensions they have taken into account. The goals are two that are partisan ("Highlight Differences between Parties" and "Demonstrate Bipartisan Agreement"); two that are institutional ("Show Deference to Others" and "Assert Congressional Independence"); and two that are policy based ("Prove Competence in Solving Problems" and "Provide Personal Policy Leadership") (p. 17). The issue dimensions are: "policy entrepreneur strength, support in Congress, public opinion, programmatic costs and discretion, and the presence of a triggering event" (p. 23). Bader uses these very effectively. They provide an organizational structure for the book and enable the author to summarize a great deal of information in a series of informative tables and charts.

Bader's research effort is truly impressive. He conducted more than 70 interviews, carefully tracked a number of legislative histories through the *Congressional Quarterly,* and sifted through materials in several archives. One critical stage in the development of his model, however, is problematic. This is the specification of the final list of leadership priorities for the five congresses (91st, 94th, 97th, 100th, 101st), which are the main focus of his study. A central question is: What can be taken as evidence that an issue was a leadership priority? In the case of the Republicans in the 104th Congress, the answer is simple: a public document, "The Contract with America." Democrats in the earlier congresses, however, were usually more circumspect about their legislative priorities. Bader explains that in such cases he relied on his interviews with leaders and their staff as well as *CQ* and archival records to identify the priorities. Fair enough, but there were a number of instances in which congressional Democrats published agenda documents or made public statements about their priorities.

The one with which I am most familiar is the "Legislative Agenda of the Senate Democratic Conference," released to the public on March 17, 1989 (101st Congress). It was sponsored by Senator Mitchell, approved by the conference, and listed a number of specific priorities. Some appear on Bader's list for the 101st Congress, but many do not. It is not clear to me why a leadership document of this sort would not be viewed as a set of *chosen* leadership priorities. (Bader makes no reference to this document in his methodological appendix.) There is also a small discrepancy between Bader's list of Speaker Wright's publicly announced priorities for the 100th Congress and those identified by Barbara Sinclair ("The Evolution of Party Leadership in the Modern House," in Allen D. Hertzke and Ronald M. Peters, Jr., eds., *The Atomistic Congress,* 1992), whom he appropriately cites as an authority on Wright's record. (She lists increased education funding, but Bader does not.)

Bader addresses this aspect of his methodology when he explains why he did not list wage and price controls as a priority for the 94th Congress, even though these were mentioned publicly by several leaders. "I made a judgment that this should not be considered a true priority because no one in Congress made an effort to turn this into concrete legislation" (p. 229). It seems, then, that choices about legislative priorities can only be recognized in retrospect and require certain steps be taken toward their realization. This approach rules out the possibility that leaders may make certain issues part of their agenda but then fail to follow up with identifiable actions. This seems to me to bias the analysis toward the finding that leaders are effective in realizing their strategic goals. Indeed, Bader's analysis of the five Democratic-majority congresses ends with "a very strong case that party leaders have tremendous success in seeing their priorities through the process" (p. 163).

Bader's method of identifying leaders' priority choices also injects a tautological element into the overall framework. If leadership goals are by definition purposive, then it must be possible to link the issues they have chosen to their goals by identifying actions taken to achieve them. Thus, only issues that were acted upon can be said to have been chosen by these purposive agents. Therefore, frivolous choices by leaders are assumed away, and half-hearted or ill-conceived choices are likely to go undetected.

I also was not persuaded by Bader's argument that the "Contract" is a case of leaders making choices about priorities that is similar in most respects to what Democrats did in prior congresses. This understates the significance of the main difference: Republican leaders made their choices *before* an election in a very public way and then stayed with their choices after the election. Congressional Democrats in the modern era have never done anything like that.

Politics or Principle? Filibustering in the United States Senate. By Sarah A. Binder and Steven S. Smith. Washington, DC: Brookings Institution Press, 1996. 247p. $38.95 cloth, $16.95 paper.

Fred R. Harris, *University of New Mexico*

The Senate of the United States is both powerful and peculiar: powerful because its authority is equal to that of the House of Representatives and then some, peculiar because it is not a majoritarian institution.

The Senate is not popularly based, of course: Each state, regardless of size, has two senators. And internally, a Senate minority can thwart a majority through what senators politely call "extended debate," a filibuster. Nothing can be done about the first of these antimajoritarian peculiarities of the Senate. But this well-researched and well-reasoned book shows that something can and should be done about the second.

Senate Rule 22 permits a minority of senators to talk a measure to death unless three-fifths of the whole Senate, 60 senators, vote cloture and, thus, end debate. Donald Matthews, in his classic *U.S. Senators and Their World* (1960), found that in an earlier Senate a powerful internal norm he called "reciprocity" caused senators to refrain from using their full filibuster and other obstructionist powers to the maximum. Matthews wrote that it was general senatorial observance of this norm that allowed the Senate of that time to act.

Since then, the Senate's external environment has greatly changed, and this, in turn, has wrought great internal change in that body. America's presently wide and more rapid communications, its much higher standard of living, and its greatly increased average educational level, taken together with what amounts to a nationalization of political issues, interest groups, and Senate campaigns and financing, have put the Senate under greater public scrutiny and outside influence, have individualized internal Senate power and made the chamber much more partisan, have dictated that senators can no longer "go along in order to get along," and have produced a Senate in which members are pressed, now, to take almost any issue "to the wall," to use all their powers, including the power to filibuster and otherwise stubbornly obstruct Senate action that they and their backers do not like. While today's Senate is more representative, it is less responsible, less able to act.

No present unwritten rule of the Senate, then, restrains today's senators from excessive filibustering. So, the authors of this book think that the Senate's written rules must be changed to do so. Specifically, they feel that Senate Rule 22 should be amended to provide that, after a fulsome and reasonable time for Senate deliberation and extended debate, a simple majority of senators can finally end all talk and vote a measure up or down.

Arguing for this rule change, the authors carefully refute the claim that Rule 22 exists and survives because of principle, not politics. It is not true, they show, that the framers envisioned a special role in our governmental system for a national Senate with supermajority requirements and extended debate; in fact, the founders preferred the same simple-majority rules for the Senate as for the House. Nor is it true, the authors document, that a principled commitment to free speech caused the great Golden Age Senate leaders of the nineteenth century to oppose rules to limit debate; a majority of them favored such restrictions.

The authors also demonstrate that it is not true that, though use of the filibuster has lately been trivialized, it was historically employed only in regard to great sectional conflicts, like slavery and civil rights; trivial and partisan filibusters have long flourished, and reform has always been blocked by partisan and policy interests, not procedural principles. Neither is it true, according to the authors, that measures favored by congressional majorities have eventually won out, despite the filibuster, and that the ultimate effect of resultant Senate talk and delay has been to moderate public policy in the interests of popular majorities. Filibusters have killed great numbers of good legislative initiatives that the public backed, and no essential connection between supermajority requirements and policy moderation has ever been apparent.

Finally, the authors show that it is not true that Senate majorities have consistently opposed changing the rules to weaken the right of extended debate. A minority of senators have on several occasions prevented just such majority-approved change. In fact, the Senate has on occasion imposed permanent statutory debate limits on itself when these have been seen to serve the Senate majority's policy interests—as on today's budget resolutions and fast-track trade legislation.

A great barrier to changing Rule 22 is the disputed but still flourishing theory that, unlike the House of Representatives, the Senate is a "continuing body" because only one-third is elected each two years. Thus, it is said, unlike the House, the Senate always has rules, and any attempt to amend those rules, therefore, must be made within the existing rules. This means that a two-thirds vote in the Senate would be required to change Rule 22, or any other Senate rule, and such a proposed amendment would itself be subject to a filibuster.

Rule 22 will only be changed, the authors write, when the public becomes adequately outraged that a Senate minority is blocking a popular piece of legislation; when a sufficient Senate majority becomes frustrated because a minority of senators is obstructing on an issue of substantial importance; or when a large enough majority of senators come to feel that their own personal and political goals are being undermined by an unreasonably obstructionist Senate minority. No great optimism is expressed in this engaging book that any of these prerequisite conditions, or Senate filibuster reform, will soon be spotted on the near horizon. The book does, however, cut the legs of principle out from under Rule 22's defenders and thereby makes change in the rule seem more possible than before.

It Takes a Nation: A New Agenda for Fighting Poverty. By Rebecca M. Blank. New York: Russell Sage Foundation; Princeton, NJ: Princeton University Press, 1997. 340p. $29.95.

Lawrence M. Mead, *New York University*

Ever since poverty became a national issue almost 40 years ago, research and writing about it have been dominated by academic economists, mostly of a liberal persuasion. Their work, based on advanced econometric and evaluation techniques, is among the most prestigious in the social sciences. Rebecca Blank, who heads a national research center on poverty at the University of Chicago and Northwestern University, is a current leader of this group.

Primarily, *It Takes a Nation* is a compilation of the economic research on poverty intended for noneconomists. Thorough chapters describe the poor population, its connections to social causes such as unwed pregnancy and the economy, programs such as welfare that try to alleviate poverty, and the evaluation record of these programs. In this summarizing role, the book is outstanding—clearly written, solidly documented, almost error free, and reasonable. It should gain course adoptions from public policy teachers seeking good overviews of antipoverty policy. Blank reproves conservative commentators for suggesting that poverty is all due to unwed pregnancy or nonwork by poor adults and that federal antipoverty programs have failed. She argues that it is harder to overcome poverty than twenty years ago because more of the poor live in female-headed families, whose potential for employment is limited, and because falling real wages for the low-skilled have made it tougher for the poor to work themselves out of poverty. Yet, welfare and other social programs do reduce need. Blank recommends improved assistance, assured child support payments, and other income supplements that would raise income while also motivating and—for the employable on welfare—requiring work effort. She rejects the notion, peddled by some conservatives, that private charity could take over care of the poor from government. She opposes recent Republican proposals to block-grant welfare to the states because this would unduly reduce federal funding and oversight. Even as a conservative, I found much of her analysis persuasive. This advice was decisively rejected in last year's welfare reform, however. The Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) did, indeed, block-grant welfare and cut funding while levying unprecedented work requirements on state aid programs. More broadly, as Blank notes, antipoverty policy is tending toward narrow programs that either give aid in-kind (food stamps, housing, and health services) or couple it with demands that recipients work or stay in school as a condition of aid. The trend rebukes the entire approach of the antipoverty economists, who generally have advised giving new benefits and opportunities to the poor while downplaying "deservingness" issues. The difficulty is not only that most economists are more liberal than most politicians and voters. They also have neglected key, noneconomic dimensions of the poverty problem. One of these is psychology. Despite all the research Blank cites, neither she nor other economists provide a persuasive account of why adults become poor long-term. Economic research correlates low income to antecedent conditions, such as low education, but it tends to be atheoretic. It does not truly explain why adults adopt lifestyles of unwed pregnancy and nonwork that are self-defeating.

Blank suggests that recent lower wages for the low-skilled make it rational for them not to work, but in response to stagnant wages most Americans have worked more in recent decades rather than less. Forty years ago, poor adults worked at much higher levels despite wages lower than today's. Those facts make it implausible that better benefits and incentives would really raise work levels. The conservative view that work must be enforced is more persuasive, and that is the premise of work requirements. This interpretation is consistent with much that sociologists write about poverty, which suggests that poor adults want to work but need both help and outside pressure to do so. While Blank partly endorses enforcement, she does not propound a view of poverty psychology consistent with it.

Blank and other economists also have a tin ear for the politics of poverty. They attribute recent conservative trends to misinformed hostility to the poor. But if the public were simply hostile, we would have no antipoverty programs. According to public opinion studies not cited by Blank, the public combines upset at the lifestyle of many poor adults with a serious desire to help needy families. Accordingly, if government were seriously to address the conduct issues in poverty, particularly nonwork, it could gain a mandate to be more generous to the poor. That is what is happening in Wisconsin, where a radical reform to base all aid on work is coupled with unprecedented health and child care subsidies for the working poor.

Finally, the economic view tends to neglect public administration. Most economists study poverty by analyzing academic databases on their computers. Few have much direct contact with the programs they discuss, and they generate few findings of use to the legislators and operators who actually deal with poverty. To her credit, Blank realizes the importance of recent mandatory work programs in welfare, the most promising of recent antipoverty policies. She gives shrewd advice about how best to operate them. But she appears to base this mostly on good judgment and a handful of site visits to leading programs. She neglects the substantial literature on the implementation of these programs, much of it by political scientists.

While Blank discusses politics and institutions to some extent, they lie at the periphery of her argument. To academic technicians, they are the stuff of art rather than social science. In fact, these dimensions are crucial to a revived antipoverty offensive. For welfare officials, reform poses primarily an administrative challenge—building a structure able to enforce work for recipients while aiding them. Similarly, improving law enforcement and education in cities requires rebuilding those institutions. In a statement many conservatives could have written, Blank argues that only the entire community, not just government, can fend disadvantaged teenagers off from ruinous early pregnancy and school failure. This suggests that the innermost solution to poverty is not changes to benefits and incentives but the reconstruction of social authority in ghetto areas. That is a task mainly for politicians and public administrators, and how to do it ought now to be the main subject of antipoverty research. Because of its overtures in these directions, Blank's argument sometimes seems like a butterfly struggling to escape from the chrysalis of economics into the wider fields of statecraft.

The Constitution of Interests: Beyond the Politics of Rights. By John Brigham. New York: New York University Press, 1997. 224p. $37.50.

Constitutional Rights and Powers of the People. By Wayne D. Moore. Princeton, NJ: Princeton University Press, 1996. 296p. $39.50.

Mark Tushnet, *Georgetown University Law Center*

John Brigham and Wayne Moore chart parallel paths through constitutional law that seem likely to lead future scholars to conclusions about the role of constitutional law in public life that differ dramatically from those that now predominate. Their books provide exciting programmatic statements and, though each offers flawed examples of how to execute the programs, are important because they describe novel ways of thinking about constitutional law that deserve to have great influence.

Brigham hopes to develop a "constitutive" theory of law and the politics of social movements: "Law and legal forms constitute social relations and political practice by delineating possible movement action and determining movement practice" (p. x). He uses sketches of four social movements to support this theory: gay rights, American legal realism, the movement toward informalism (alternative dispute resolution in particular) in procedural law, and the movement among some feminists to develop new ways of regulating the distribution of sexually explicit material. So, for example, in the early stages of public response to AIDS, gay activists invoked the idea of rights to protect their community against intrusive regulation. Gays claimed rights "in an expression of their solidarity and concern about the repression they felt sure would follow any assertion of authority from the state" (p. 47). But the degree to which activists made rights claims depended on the strength of the community. In San Francisco, the politically well-organized gay community transformed public health policy, which made it less important to that community that its claims be articulated in terms of rights.

Brigham's case studies are too truncated to "prove" his theory. Each chapter offers readers some striking insights. The discussion of the antipornography movement, for example, makes the nice point that its politics has been shaped more by its need to deal with a popular ideology of free expression—a popular libertarianism inconsistent with much public policy in other areas—than with formal free speech doctrine, which can readily be accommodated to the movement's goals.

Brigham's concluding chapter describes the role of intellectuals in the dialectic between movement politics and rights. As he sees it, "intellectuals interpret law to social movements" (p. 131) and thereby shape power relations. The gay rights vision saw law "as a protective barrier against discrimination," while the legal resistance to the antipornography movement arose because intellectuals' "fear of mass politics reinforce[d] the idea that radical opposition to law is reactionary" (p. 133).

The idea that law and legal forms "constitute" movement politics is an important one, but it needs more fleshing out than Brigham offers. He resists a merely instrumental interpretation of law's role in movement politics: Law is more than a strategic instrument for accomplishing goals determined without regard to existing legal forms. Yet, the ways in which law constitutes movement politics are not well identified. One version of the image Brigham offers of the process appears to be this: Intellectuals examine the law, defined in positivist terms as statutes, constitutions, and court decisions, and offer their interpretations to the groups with which they are affiliated. (Brigham's allusions to opposition to the antipornography movement suggest his awareness that movement members themselves cannot be insulated from interpretations provided by less sympathetic intellectuals as well.) Often the interpretations are merely strategic, although sometimes the intellectuals' interests may structure their interpretations. Movement members then assimilate the interpretations. They may, for example, reshape their understanding of the movement's goals in response to what initially came to them as strategic advice. In this way, law is "a part of the way we think and act" (p. 27). Brigham's case studies show that something like this probably does happen, but a constitutive theory of law and movement politics requires much more detailed examination of particular movements than Brigham's brief chapters provide. It seems worth emphasizing, as well, that at least part of the image I have described appears to implicate a distinctive role for intellectuals as such, that is, as a group allied with but to some degree independent of particular social movements. As Brigham points out, Gramsci's observations about organic intellectuals clearly bear on the analysis, but neither Gramsci nor Brigham provides a full account.

Brigham sometimes suggests a somewhat different image of how law constitutes movement politics. Here we would not take a positivist view of law but would directly examine what Moore calls unofficial understandings of law. The idea would seem to be that the term *law* has special cultural valence in the United States, and movement activists and members are drawn to using it as a way of articulating demands that in other societies would seem less legal than cultural. Like Brigham, Moore seeks to understand how "the people at large may play roles in creating and sustaining constitutional norms—including 'legal' norms that do not fit readily within professional narratives" (p. 11).

Moore's book contains two components that fit awkwardly together. He opens with chapters discussing official conceptions of citizenship, as revealed in the *Dred Scott* decision, and unofficial ones, in Frederick Douglass's constitutional vision. Moore sets up a general tension between a formal conception of constitutional law, which neither makes demands on citizens nor takes their contributions to its creation as significant, and a participatory conception, which does both. He uses this distinction to criticize Robert Cover's influential analysis of law as necessarily jurispathic, arguing that Cover failed to appreciate how unofficial interpretations of the law offered by popular movements can fairly be regarded as law despite their lack of official ratification. Douglass "exemplified forms of active self-constitution" (p. 63) by acting politically on his own unofficial conclusion that he was indeed a citizen. Moore then examines the ways in which the people act "through government and independently," as the title of his third chapter puts it. Here he examines the amendment processes—the formal process of Article V and the informal processes stressed by Bruce Ackerman, Akhil Amar, and William Harris. Elsewhere Moore describes the role of state legislatures and executive officials in response to *Roe* v. *Wade* as an example of the people acting "through voting," though not, it seems to me, independent of government writ large. In a sense, Moore's work constitutes a doctrinal counterpart to Brigham's more sociological analysis.

Moore's second theme sometimes obscures this more important one. A large part of the book offers an account of the relations among the powers of governments and the rights of the people, describing various ways of conceptualizing the structure of those relations. For example, Moore shows how a right to privacy can be understood either as a "categorical absence of governmental authority or as capable of otherwise limiting exercises of collective power" (p. 230). Moore's examination of these alternative conceptual structures leads him to create a number of diagrams mapping possible relations. These may work in a classroom that has been devoted to developing the framework, but I found them largely unhelpful. The bulk of the second half of the book examines the standard constitutional controversies covered in basic courses to show how "legislative and executive officials have promoted collective ends and reinforced or limited popular choices" (p. 172). Here I believe Moore may have fallen into the "first book" trap of using the book to show how he presents the standard content of an introductory historically oriented course in constitutional law in a distinctive way. His chapter on the controversy over the Bank of the United States offers a reading of *McCulloch* v. *Maryland* that seems to me conventional, at least among law professors. He fits his discussion of modern federalism cases into his framework by characterizing them as demonstrating, in a way reminiscent of Herbert Wechsler's classic argument, that the states act through the national government "to *secure* conditions in which they could effectively exercise their reserved powers" (p. 211). Economic rights, Moore says, remain constitutional even though they are now primarily enforced by legislatures and executive officials, not judges.

All this is sensible enough but not nearly as exciting as the basic insight that drives the discussion: We can no longer "assume that constitutional limitations [are] coextensive with judicially enforced limitations" (p. 213). According to Moore, there are "other types" of processes, "including those provided by principles and structures of legislative and executive representation and popular accountability" (p. 213). Once again, standard constitutional law exerts its pull, for none of those processes seems to me fairly distinguishable from ordinary official law processes.

Future work along the lines Brigham and Moore lay out will have to focus on the precise ways in which unofficial interpretations of law, by social movements of many types, end up constituting popular legal consciousness and then, as Brigham argues, work their way into, and help provide structure for, official legal institutions. It is an extremely promising project, and Brigham and Moore will surely be regarded in the future as among the founders of an important way of looking at constitutional law.

Affirmative Action and the Stalled Quest for Black Progress. By W. Avon Drake and Robert D. Holsworth. Urbana: University of Illinois Press, 1996. 232p. $34.95 cloth, $16.95 paper.

Abigail Thernstrom, *Manhattan Institute*

What a surprise: a book on affirmative action that actually has something new and illuminating to say. No thread-bare arguments, no tiresome polemics, no cute little anecdotes in the service of one political agenda or another. Drake and Holsworth offer solid scholarship presented in low-keyed, clear, unpretentious prose. Their book is a must-read for scholars interested in race and public policy.

In 1989, in *City of Richmond* v. *J.A. Croson Co.,* the Supreme Court struck down Richmond's minority set-aside, which required all white-owned construction firms engaged in municipal work to subcontract at least 30% of the dollar amount of the prime contract to minority business enterprises. Classifications based on race were deeply suspect, said Justice Sandra Day O'Connor, writing for a majority of five: "The framers of the Fourteenth Amendment . . . desired to place clear limits on the States' use of race as a criterion for legislative action."

The *Croson* decision sets the stage for Drake and Holsworth. What, they ask, was the political context in which the set-aside ordinance was passed? Whose interests did it serve? Did it turn out to benefit Richmond's black community, as hoped? How did whites react to the policy? And given the vulnerability of affirmative action strategies to attack, is there perhaps an alternative to race-based policies that may better promote black progress?

Richmond has been a black-run city since 1977, when African Americans became a majority on the city council. In fact, black power in the city predated the passage of the 1965 Voting Rights Act; by the early 1960s, black voters were a force to be reckoned with. But as Drake and Holsworth make clear, they were not part of one big, happy political family. And in Richmond, the black majority on the council, from whose ranks the mayor was chosen, soon split in two. Moreover, black ascendancy did not change life for most ordinary African-American voters. The usual litany of urban problems—drugs, violence, low levels of academic performance, and high levels of poverty in the inner city—were as prevalent as in any white-run city.

The set-aside ordinance did enrich a few black businessmen—very few. But its main effect was a marriage of political convenience, a working alliance between the economically privileged of both races. The white business elite signed on to a piece-of-the-pie for blacks in order to polish its image as socially conscious and secure support for the downtown revitalization it wanted. Black politicians used the bargain to suggest their own importance to low-income constituents for whom the set-asides actually did little. Neither cared whether the policy in fact provided real economic benefits, which it did not.

In fact, Drake and Holsworth argue, the black leadership cannot be credited with much in the way of economic progress of any sort. Their point is thus one that extends far beyond the question of set-asides. Black city employment went up after blacks gained control of the municipal government, but it had been rising at the same rate in the five years prior to 1977. More was spent on schools, but the educational system did not improve. The city council promised to revitalize Jackson Ward, a downtown black neighborhood, but the project never got off the ground. Moreover, the residents needed not the African-American business institute that was planned but ordinary grocery stores.

Not even the introduction of single-member districts—a much-celebrated civil rights strategy—worked out well. Ward-based candidates showed little interest in debating citywide issues affecting the economic health of black neighborhoods. Nor were they particularly enthusiastic about black registration drives; as Drake and Holsworth put it, "they saw little advantage to bringing into the political arena a new group of participants who may or may not have felt that their interests were served by the current order" (p. 108).

In the Richmond political climate, corporations had nothing to lose and much to gain by race-conscious hiring. Although white-owned construction companies were not happy with the set-asides, they exercised little influence in the city's business community, and their cause stirred minimal political interest among the voters. Their preoccupation with school integration and other more immediate concerns, coupled with the parochial nature of ward-based politics, meant that set-asides never became a matter of high political visibility.

Black politicians thus had a free hand with respect to set-asides, but these were never worth the political and moral capital that the civil rights community spent in defending them, Drake and Holsworth imply. In Richmond (and elsewhere), the real economic problem is one that preferences cannot address: Much of the middle class, black and white, has moved to the suburbs. And thus a depressingly familiar set of urban problems remain untouched by race-conscious strategies.

For this reason, the authors do not join the hand-wringing spokesmen for the civil rights community who see a catastrophic defeat in the election of a Richard Riordan or a Rudolph Guiliani in cities in which blacks once held the mayor's seat. They view these elections as a potential wake-up call—the start of a new day in which the politics of development replaces that which stresses affirmative action and black officeholding. But here the book becomes quite fuzzy. And after so much splendid analysis, the lack of clarity at the very end is disappointing. One other small complaint: The book is marred by some minor errors that should have been caught. For instance, Mayor Riordan is given the wrong first name, as is the writer Jim Sleeper.

The Face of the Nation: Immigration, the State and National Identity. By Keith Fitzgerald. Stanford, CA: Stanford University Press, 1996. 285p. $39.50.

Rodolfo O. de la Garza, *University of Texas*

This study develops a theory of "improvisational institutionalism" intended to explain American immigration policy. The author argues that this is needed because the major empirical theories that guide research on this topic ignore the role that the state plays in the policy process and thus yield a distorted and incomplete understanding of this policy. Fitzgerald claims that by incorporating the role of the state, his theory explains and gives coherence to what is usually described as a set of incoherent and contradictory policies.

The study offers several examples to illustrate that neither class-based nor pluralistic perspectives can explain key aspects of immigration policy. For example, although undocumented immigration may be seen as a mechanism for creating a docile work force, too many such workers create instability by generating frustration among the working class, which sees its upward mobility blocked. Thus, class-based theory predicts that business groups will oppose illegal immigration rather than support it as they do. Relatedly, given the major role that organized interests such as employers play in pluralistic analysis, that legal immigration policy is not driven by labor-market concerns is evidence of the inability of pluralist theory to explain immigration policy. Further proof of the inadequacy of class-based and pluralistic arguments is that neither can account for the development and evolution of refugee policy. The major reason for these deficiencies, according to the study, is that neither theory includes the state as a major independent actor.

Fitzgerald describes immigration policy as consisting of three distinct segments: front-door immigration (such as we associate with Ellis Island), back-door immigration (undocumented agricultural laborers), and refugees. The development of these segments is integral to the transformation of immigration policy from being decentralized and dominated by state and local governments to a national policy, which according to the author is now dominated by the state. The study describes each segment as having an identifiable policy network that includes a distinct set of actors who use a particular rhetoric to advance their goals, and Fitzgerald uses congressional testimony to show that the membership of these networks and the rhetoric they use have evolved in keeping with changing national values and political priorities.

The study also contends that each policy network has remained uninvolved with the other networks even though each group's efforts affected the interests of the other two. For example, the nativist-dominated network that led the effort to restrict front-door immigration to Europeans did little to combat back-door immigration or refugee policy even as these undermined the goals of the legal immigration policy the network had fought so hard to establish. This, the author claims, is partly because the actors involved in the latter two did not refer to these as "immigration" policies. Instead, one was discussed in terms of temporary labor policy and national economic needs, and the other was defended with reference to the nation's democratic and humanitarian ideals.

The existence of three policy networks pursuing such diverse objectives leads to the conclusion that immigration policy is disjointed and contradictory. Such a conclusion, however, ignores the role of the state. Fitzgerald argues that once the state became established as an actor in immigration policy in the 1920s, it pursued its own interests both by developing a specific policy segment to serve its distinctive needs, that is, refugee policy, and by influencing the design and implementation of front-door and back-door policies to ensure that neither of these damaged state interests. Thus, the author claims that "improvisational institutionalism" explicates how the state has become the dominant actor in immigration policy, and he shows that state interests link the three policy segments and bring coherence to immigration policy.

The study is less successful than it claims. Most significantly, its theoretical goal is unrealized. It does make clear that the state, or specific sectors within it, have a policy role that is independent of societal actors. As the author's review of the literature documents, however, this is no longer a revolutionary finding and therefore does not merit the trumpeting it receives here. This is especially so given that the state's role in immigration policy has been carefully analyzed by Kitty Calavita (*Inside the State: The Bracero Program, Immigration, and the I. N. S.*, 1992) in a study that is conspicuously absent from an otherwise extensive bibliography.

More debilitating is Fitzgerald's reliance on the claim that refugee policy is "arguably the most consequential" (p. 55) part of immigration policy. Throughout, his tone suggests that refugee policy is the major source of immigrants, and in a somewhat obtuse statement he declares that "more recent immigrants have been refugees than from any other specific category of sanctioned or unsanctioned groups" (p. 18). It is unclear why the author compares refugees to subcategories of legal immigrants unless it is to buttress what would otherwise be an untenable claim. Whatever the motive, the effort fails. In every year from 1988–95, the INS *Statistical Yearbook* reports that subcategories of legal immigrants exceeded the number of refugees and asylees, and there can be no doubt that this was also true in earlier years. Moreover, when comparing refugees to legal immigrants as a whole, the former pale in comparison. According to the most recent data (1986) that Fitzgerald himself presents, legal immigrants outnumber refugees six to one, and INS data show this ratio also held from 1988 through 1995. Furthermore, illegal immigrants are estimated to have exceeded 200,000 annually for more than a decade, a total that also exceeds INS figures for refugees and asylees.

Relatedly, the study asserts that foreign policy (i.e., state) objectives played a greater role in restricting immigration between 1929 and 1941 than has been previously recognized. Consuls, according to the author, used the restrictions against potential "public charges" to reduce the number of immigrants coming to the United States in order to prevent public demands for new immigration laws that could conflict with foreign policy goals. Given the nativist restrictions already in place, the Depression, and the war-related mobilizations in effect for much of the period, this claim requires much more systematic documentation than it receives.

Numerous other problems also diminish the volume's value. The title misleadingly suggests the book will concentrate on how immigration affects demographics and, perhaps, national values, but its focus is on the state's role in determining immigration policy. It mistakenly describes the Alien and Sedition Act of 1798 as immigration rather than as immigrant policy, that is, it targeted naturalization rather than who entered the country. The discussion of Mexican immigration lacks detail. For example, it makes no mention of how few Mexicans there were among immigrants to the Midwest and Northeast who were the object of the nativists' protests. The author should have considered whether this helps explain why Mexicans were not targeted by the National Origins Act of 1924. Also, the linkage between the bracero program and undocumented Mexican immigration would have benefited from more thoughtful analysis and from the insights developed in Calavito's *Inside the State*. At a more mechanical level, three key tables (5–7) intended to illustrate the changing composition of witnesses at congressional hearings are mislabeled and instead suggest that all witnesses were government employees.

Finally, the theoretical arguments are unnecessarily dense and jargon laden. Moreover, Fitzgerald engages in an unnecessary and counterproductive epistemological duel with positivistic social science to defend his method. He would have been much better served if he had avoided grandiose claims and recognized that his work fits well into established patterns and makes useful incremental contributions to our understanding of immigration policy and the policy process.

National Elections and the Autonomy of American Party Systems. By James Gimpel. Pittsburgh, PA: Pittsburgh University Press, 1996. 241p. $45.00 cloth, $19.95 paper.

Peter F. Galderisi, *Utah State University*

In this well-researched volume, James Gimpel reassesses V.O. Key's now classic claim that party competition leads to party unity (*Southern Politics in State and Nation*, 1949; *American State Politics*, 1956). Adding an interesting perspective to the study of the fragility of party cohesiveness in modern U.S. politics, Gimpel attempts to explain the anomalous condition of party disunity in western states with high levels of bipartisan conflict. Regardless or in spite of the standard array of rules governing interparty and intraparty warfare, greater levels of disunity will exist in those states in which local party coalitions are different from or autonomous of the ideological and social coalitions that define national party divisions.

This "electoral incongruence" manifests itself in several ways. The issue divisions that help define what E. E. Schattschneider (*The Semi-Sovereign People*, 1960) called "the mobilization of conflict" in a particular state can be different from those mobilizing national electorates; the issues can be the same, but with a differing ideological point of demarcation for state and national electorates; or a combination of the two. In addressing this electoral incongruence, both generally and through a series of richly detailed case studies, the author provides a carefully documented assessment of the potential effects of a federalist electoral system on party unity. The greater is the difference between state and national two-party divisions, the more likely elections will be candidate-centered than controlled by party organization, the greater the certainty that nominations for major party office will be hotly contested, and the more likely state politics will be categorized by electoral volatility, both across time and across elections.

As with any volume seeking to tackle so complicated a set of subjects, certain methodological concerns can be raised. Given the centrality of the case studies to the analysis, greater care could have been taking in justifying the selection of the states chosen (a subset based upon Mayhew's categorizations in *Placing Parties in American Politics*, 1986), although those included do fill out the author's typology nicely. Gimpel (and he is not alone on this) at times has difficulty convincingly separating cause and effect or demonstrating directionality, a classic chicken or egg dilemma. For example, the western states, generally categorized as two-party competitive yet lacking unity, indeed have cultural histories and economic concerns that would be more likely to yield statewide political divisions incongruent with the national divisions most congruent with older, established eastern party systems. Yet, aside from a quick dismissal, little is said in any consistent fashion of how early definitions of the rules of intra- and interpartisan conflict (such as ballot access), themselves a result partially of early coalitional divisions, may have permanently affected the ability of party organizations to control their members' candidacies or positions.

The very definition of party organizational unity is perhaps at the core of this dilemma. In many if not most states, party organizations have more resources at their disposal than at any time in the past, yet we consider parties to be weaker as definers of the electoral order. If organizational strength is measured mainly in terms of the consistency of campaign issues and the control of candidacies, as the author does, then parties may indeed be weaker. But if measured as informal organizations seeking to coordinate campaigns and achieve victory for their candidates, however chosen (e.g., see Joseph

A. Schlesinger, *Political Parties and the Winning of Office*, 1991), then parties may actually be stronger, and state and national party organizations may be less at odds than we think.

What Gimpel's work does tell us is that the generally candidate-centered nature of U.S. politics, stronger in certain states than others, can be either reinforced or moderated by the level of complexity and incongruence of state and national political divisions at any point in time. Whether party organizations coordinate activity among like-minded or contrary-minded individuals will be at least partially a function of this level of coalitional disparity; and coordination may indeed be easier if like-mindedness consistently prevails. Whether contrary-minded candidates do not cooperate, or whether that lack of cooperation inhibits party-building activity, as the author suggests, is still very much a subject of debate.

In total, however, this volume is a welcome addition to the evolving research on state party environments, including works by Key (1949, 1956), Cotter and others (*Party Organizations in American Politics*, 1984), Mayhew (1986), and Erikson and others (*Statehouse Democracy*, 1993), as well as the study of the coalitional structure of partisan politics generally. It is consistent in its treatment of evidence; artistically varied in its use of anecdote, surveys, and ecological data; and elucidating in its richly detailed case studies. Reading this volume will help students of partisan politics understand the importance of demographic change and stability on electoral outcomes as well as the timing and depth of realignment. In describing and analyzing the political variant of western exceptionalism, Gimpel offers an insight into the evolving politics of all states as well as an understanding of national party politics of the past, present, and future. The true test of his hypotheses may well lie in that future. If western political concerns begin to match more closely national partisan debates, as is likely to be the case, it will be interesting to see if party unity, as described by the author, begins to increase in that region.

Race, Campaign Politics, and the Realignment in the South. By James M. Glaser. New Haven, CT: Yale University Press, 1996. 229p. $28.50.

Thad Beyle, *University of North Carolina at Chapel Hill*

This study looks at the "politics" side of our discipline, specifically, recent politics in the South. Glaser seeks to explain two separate yet intertwined political puzzles on the changing politics in the South of recent decades. His South is the eleven states of the former Confederacy: Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North and South Carolina, Tennessee, Texas, and Virginia.

Glaser sets up this exploration by examining several trends in these states over the 1952–92 period: the party identification of both all southerners and a subset of white southerners; the presidential electoral vote; the percentage of Democrats in Congress; and the percentage of Democrats in the state legislatures. While Democrats lost considerable ground to Republicans over the period in terms of the presidential electoral vote and in the party identification of many southerners, Democratic losses in congressional and state legislative delegations did not match these trends.

The first puzzle concerns the inability of the Republican Party to capitalize on its growing strength in presidential elections by winning contests farther down the ballot. Glaser wonders why "conservatism and several popular Republican administrations did not give the Republicans the advantage one might expect in lower-level elections" (p. 23). The second puzzle concerns the role of race in the region's politics, the overriding concern which has driven Democrats from the party of heritage toward the party of Lincoln. He asks why race and racial issues do not "appear to be giving Republicans the lower-level advantage one might expect" (p. 23). He wonders if the reason is the Republican Party going wrong or the Democratic Party doing right?

To explore these questions, Glaser selected six congressional contests to see what aspects of the candidates, the campaigns and issues, the voters, and the nature or context of the district may help solve these puzzles. These special congressional elections were not held when other elections were in process (presidential, gubernatorial, senatorial, state legislative). In effect, they stood on their own, without coattail or spillover effects from other political contests. They were held in Alabama, Mississippi, Texas, and Virginia between 1981 and 1993 and represented a range of district types, from those with 19% black population to one with 59% black population. Glaser used on-site observation techniques for the three later races and extensive archival and interview techniques for the three earlier races.

Glaser finds several pieces or parts of the answers to the two puzzles he set out to solve. Context is a most important factor in terms of the interplay of the political campaigns, issues, and the racial composition of the district. In those districts with large black populations which made a larger contribution to the electoral outcome, Republicans tended to use racial issues to win Democratic supporters to their side, while the Democrats moved toward a biracial support base. For districts in which the black population would not make as large a contribution to the electoral outcome, the Republicans moved away from racial issues to other conservative issues of interest to white voters, while the Democrats tried to court these same white voters and used a variety of channels to hold and entice the blacks.

A second part of the solution Glaser sought is tied to the model used by each party in framing its overall approach. The Republicans tried to build on the Washington model of winning national elections and "emphasized their superior conservatism and ties to the president" (p. 131). The Democrats looked more locally and built on their long-time service to the district and "discussed opportunities to deliver federal money and federal projects to the highly dependent area" (p. 131).

Related aspects of this national-local dichotomy were the problems of party building and party maintenance. In a few words, the Democrats had a farm system with many local elected officials and potential candidates moving up the ladder and ready to seek higher office. The Republicans had to build a party in order to begin developing the range of candidates needed to win elections at all levels. In effect, the Republicans had a recruitment problem to address.

There are more pieces to the answers Glaser sought, and those noted above are often more subtle and related to even more complex answers. But as Glaser notes, his "work is that of describing and analyzing a moving target" (p. 24). And the target has moved since the time frame of his study.

A few political facts on the politics of these southern states demonstrate this. Democrats now make up only 43% of the southern delegation in the U.S. House, dropping from more than 60% in 1992. Democrats make up only 32% of the southern delegation in the U.S. Senate, dropping from more than 50% in 1992. Democrats hold slightly less than 60% of the state legislative seats in these eleven states in 1997, down from around 70% in 1992. And they only hold three of the eleven governor's seats after the 1996 elections. These facts are not presented to fault Glaser's work but to second his view that what is happening in southern politics is changing constantly, "a moving target."

We can only hope that the next student to survey the southern political scene does so with the same careful attention to what is happening "on the ground" in the politics of these states and the districts involved. Glaser's insights as to the politics of the pre-1993 period do need to be updated but with the same sense of politics and how candidates, campaigns, issues, and district characteristics interact. The type of research he conducted and presents in this book is time consuming and often difficult. But it leaves the reader with a sense that the author understood the essence of the topic addressed.

The Presidential Republic: Executive Representation and Deliberative Democracy. By Gary L. Gregg II. Lanham, MD: Rowman and Littlefield, 1996. 241p. $57.50 cloth, $22.95 paper.

Samuel B. Hoff, *Delaware State University*

The concept of political representation, so extensively analyzed as it pertains to the legislative branch of American government, is applied to the presidency in this timely and thought-provoking study. Gregg's thesis is that the evolution of the presidency, particularly in the twentieth century, has uprooted the Constitution framers' idea of the manner by which the national executive should be linked to the American people and, in turn, has replaced the precepts of a deliberative government with those of a prebiscitary, prerogative-grabbing presidential republic. Employing a variety of original source material, the work depicts how the latter transition occurred, its destructive systemic consequences, and what steps may be initiated to rectify the imbalance.

After reviewing how the Constitution imbues the president with both activity-based and symbolic representation, the author identifies six traits found in the *Federalist Papers* which defenders of the newly formulated government regarded as crucial for effective representation: filtering, or refining and enlarging public sentiment; focus on the common good; distance, meaning the need to rely on one's best judgment in an extended republic; reason; virtue; and institutional independence. In theory, the president would share the representation function with the legislature, uphold the principles of the Constitution, and limit the direct influence of the polity to sway government policy. In short, he would serve as "guardian of the process of free government as well as an active ameliorator of its product" (p. 73), according to Gregg.

The text then turns to the effect that alternative viewpoints of presidential power have had on the founders' definition of representation. On the one hand is historical distrust of executive power, exemplified by the Whigs of the nineteenth century, which places the primary role of political representation in the legislative branch. On the other hand is presidentially centered government, prescribed in prototypical form by political scientist Richard Neustadt, which assumes action is favored over deliberation, that the president is the deliverer of national policy, and that presidential influence is best understood in realistic rather than formal terms. Gregg contends that "fluctuation between these two understandings has made it even easier to embrace one institution over the other" and that they "have periodically pulled America toward one extreme or the other" (p. 113).

The latter alterations have been perpetuated by features of the modern presidency. Gregg delineates how contemporary elections, direct presidential appeals to the public, and proliferation of opinion polls together with changing institutional features—such as increasing delegation of power to the executive, the president's tendency to dominate foreign affairs, the proclivity to use weapons of the office to act unilaterally, and the growth of the executive branch—have led presidents to believe that they are the sole legitimate representative of the nation.

The Presidential Republic appears at first glance as a hybrid of Jeffrey Tulis's *The Rhetorical Presidency* (1987) and Samuel Kernell's *Going Public: Strategies of Presidential Leadership* (1993). Yet, Gregg it is more thorough in the theoretical framework developed to examine political representation than is Tulis and broader in the portrayal of the foibles facing the presidency than is Kernell. Gregg eschews the statistical tables and empirical tests found in the aforementioned research and relies instead on a meshing of political philosophy, constitutional history, and synthesis of scholarly studies on the presidency.

There are some shortcomings in this work. First, rather than evaluate the development of the presidency in stages, as does Tulis, Gregg claims that the advent of the public presidency and modern presidency coincide with Franklin Roosevelt. This perspective ignores how chief executives of the nineteenth century used several vehicles—most notably political parties—to interact with the public.

A second drawback is that the contribution of certain institutional tools to political representation is erroneously judged. For instance, Gregg states that the "essence of the veto is negative" (p. 18). In connecting the executive and legislative branches and forcing Congress to reconsider unwise or intrusive proposals, however, the veto becomes an exemplar of deliberative democracy, which President Clinton showed so vividly in his disputes with the House and Senate in 1995 and 1996. Furthermore, whereas the author remains noncommittal on the influence the item veto will have on representation, he likewise cautions against shifting more responsibility from Congress to the president. Not coincidentally, it was precisely that argument which was used by legislators who successfully challenged the 1996 item veto law in federal district court.

Probably the most glaring weakness of the study is the concluding chapter. By attempting to accomplish too much at once—summarizing the negative ramifications of presidentially dominated representation, advancing recommendations for reconstructing deliberative democracy, and proposing an agenda for future study of the topic in just twenty pages—the author has left the reader wanting more material in each section. Still, *The Presidential Republic* is an eminently well-written and effectively argued effort. Even if the solution proffered to the dilemma of presidential aggrandizement and reactions to it—a return to the representational balance envisaged by the architects of the Constitution—is more logically sound than practical or possible, the study nonetheless directs our attention toward a principle hitherto ignored in presidency research and reminds us of its proper place in a republican form of government.

Down from Bureaucracy: The Ambiguity of Privatization and Empowerment. By Joel Handler. Princeton, NJ: Princeton University Press, 1996. 269p. $29.95.

William T. Gormley, Jr., *Georgetown University*

In recent years, we have witnessed numerous efforts to curb big government. In a quest for greater efficiency, we have tried various forms of decentralization, deregulation, and privatization. Although many of these initiatives are controversial, both Democrats and Republicans have enlisted in the fight against big government. The quest for efficiency has animated most of these reforms, but some of them also seek to "empower" citizens. This is most evident in such cases as school vouchers and tenant management of public housing complexes. It is also arguably true of cooperative regulatory enforcement schemes and decisions to contract out the delivery of social services.

These diverse initiatives constitute the focus of Joel Handler's book, *Down from Bureaucracy*. Unlike other scholars, who have studied privatization or deregulation or devolution, Handler examines all the above. Unlike other scholars, who ask whether these reforms promote efficiency, effectiveness, or innovation, Handler asks whether they empower consumers, entrepreneurs, and local officials. The scope of the book is impressive. It encompasses all levels of government and a wide range of policy examples drawn from education, health care, occupational safety, housing, land use, and other areas. The spade work has been done by other scholars, but Handler does a good job of summarizing a diverse array of studies.

Also, Handler's emphasis on empowerment is refreshing. From a democratic theory perspective, we should care not only about results (e.g., lower taxes or better services) but also about the political process (e.g., stronger local control or wider citizen participation). The emphasis on empowerment is especially apt because so many of the efficiency studies are inconclusive.

In discussing empowerment, Handler draws a distinction between empowerment by "invitation" and by "conflict." In the former case, weaker parties obtain valuable resources from government agencies who decide to assist their critics. In the latter case, weaker parties attempt to extract resources from the wider community without the support of targeted bureaucracies. Handler's thesis is that empowerment by invitation is superior to empowerment by conflict. When government bureaucrats forge a cooperative partnership with the clients they are supposed to serve or the businesses they are supposed to regulate, he argues, successful empowerment is more likely to occur.

This thesis seems reasonable enough but receives only weak support due to questionable operationalization of key concepts. For example, Handler treats tenant management as an example of empowerment by invitation, school choice as an example of empowerment by conflict. Yet, many tenants' rights groups opted initially for adversarial tactics, and magnet schools have been championed by many state and local bureaucrats. Thus, one wonders whether the two concepts are discrete. More broadly, one wonders whether invitations are sometimes facilitated by early conflicts. If so, one empowerment strategy may pave the way for another.

Problems also arise when trying to identify the weaker party in a relationship. For example, Handler characterizes firms regulated by OSHA as weaker parties without really defending that assertion (p. 132). Elsewhere he asserts that state governments regulated by the federal government are stronger parties without defending that assertion either (p. 30). In fact, one can argue that OSHA requires the cooperation of regulated firms every bit as much as the federal government requires the cooperation of state governments. More broadly, one suspects that weakness is highly contextual and needs to be investigated rather than assumed.

Although Handler does not discuss the context in which particular firms or state governments are likely to be weak, he does identify three preconditions for the successful empowerment of relatively weak clients. The most important of these is an "exchange of material resources" between dependent clients and government bureaucrats. Otherwise, a win-win situation becomes impossible. In Handler's words (p. 241), "the powerless have to be in a position to contribute something of value." This insight helps explain why some empowerment efforts have been successful and others not.

The book's bottom line is somewhat disappointing because it is much more limited in scope than the big topics addressed in the first four chapters. We learn that privatization and deregulation and devolution are problematic, but are they worse than big government itself? Are they equally flawed, or is one form of decentralization superior to the others? And when exactly should we decentralize? After summarizing so many studies, Handler has earned the right to answer these questions but does not elect to do so.

Despite these problems, Handler is asking some important questions. Can we liberate the creative energies of consumers by decentralizing governance arrangements? Are state and local governments more responsive than the federal government? Is the private sector more versatile than the public sector? Is cooperation more fruitful than conflict? When is empowerment attainable?

In *Down from Bureaucracy*, readers will find neither definitive answers to these questions nor a full-blown theoretical framework to guide them. They will find some useful insights and ideas and a rich assortment of policy examples. They also will find a convincing warning that some of our solutions to big government may be as problematic as big government itself.

Continuity and Disruption: Essays in Public Administration. By Matthew Holden, Jr. Pittsburgh, PA: University of Pittsburgh Press, 1996. 291p. $45.00 cloth.

John Rouse, *Ball State University*

The underlying thesis of this work is that administration is central to politics. According to Matthew Holden, Jr., "politics, in brief, is the process of organizing power within human groups, large and small, public and private" (p. 2). Furthermore, "administration cannot be separated from politics, and no political system can be sustained when its administrative core collapses" (p. 2). The author states that politics constitutes reasoned discourse among people who share values, moral language, and capacity for action. These persons prefer to see one another as members of the same community.

The book originates from the Ransone Lectures given by Holden at the University of Alabama in 1987. The author links historical concepts and practices, current issues, and modern research in three parts. Part I describes intellectual history. Part II analyzes dogma and controversy. Part III discusses external sources of disruption.

The aim of this text is to contribute to a political theory of administration that will offer greater clarity on five intellectual and practical problems in human action. Problem 1 is explaining and foretelling the growth, stabilization, or decline of institutions that constrain the use of force, money, and information by formal administrative structures. Problem 2 is explaining the different capacities of administrative institutions to acquire and exercise discretion about the use of force, money, and information. Problem 3 is understanding the structural features of the administrative system, including bargaining-and-command relationships between superiors and subordinates; the requirements of "field administration"; and decision making about the extent of central control, devolution, and negotiated power sharing. Problem 4 is administrative initiative in policy making. Problem 5 is the extent to which our knowledge permits us to understand administrative and organizational design as predicting, as forecasting, or merely as prophecy.

Why did political science cease to study administration? Political science has focused on undergraduate teaching. Money was not available for researching administrative questions. Perhaps most important, however, methods for observing, explaining, and anticipating the organization of power were not considered scientific in a way that commanded respect. The "sophisticated" political scientists looked down on the "unsophisticated" public administrationists.

As political science surrendered public administration, three themes emerged. First, is bureaucracy rational? Of what sort of rationality? Second, does bureaucracy tend toward elephantine complexity and lethargy or rigidity, rendering its members incapable of intelligent action? Is bureaucracy essentially a pathological phenomenon? Third, does bureaucracy tend toward despotism so that it is inevitably in conflict with law and/or democracy?

Dogma is the alternative to empirical theory. According to Holden, "dogma means a belief that one is prepared to take responsibility for deciding not to examine" (p. 78). Executive leadership dominates American political thought. The primacy of presidential leadership was a moral defeat for the legitimacy of the Congress. Political scientists cite the power of the president in his role as representative of all the people.

A tension exists between administrative theory and political dogma. Dogma often precludes new thought or new interpretation of data from experience old and new. If so, then dogma frustrates the best efforts of empirical analysis. Holden addresses dogma and the politics of administrative law. The latter forces us to think about change. The U.S. bar and its demand for judicial review of agency actions turned administrative law from an academic idea to a practical result. The question now is how to think about and evaluate the workings of our modern administrative law system.

Holden claims that interests are fundamental and do not die because of a change in positive law. New interests are always emerging. The system cannot be limited to initial purposes. Administrative law addresses the liability of public officials and rights of government employees. The Administrative Procedure Act of 1946 compromised between giving government agencies wide administrative discretion and curbing power by an administrative court. The APA is part of the political and administrative culture of the United States.

As a matter of logic, the administrative law world is half closed. In this arena, a relatively stable set of participants, bound by complex rules and a language that only they speak and comprehend, engage in rule making. As a function of political partisanship, the system has a quality of being half open and a close connection to electoral, partisan politics.

Holden offers tributes to Leonard D. White and John M. Gaus in a probe of politics, technology, and administrative history. Modern technology affects how persons and practices adjust to one another. This adaptation is ecological. Ecological adaptations create new structures of interest. The politics of the office and of management change. Administrative communication confronts span of control and depth of control problems and is both social and cultural.

Finally, the author probes public administration and the plural society. Ethnic identity theories in public administration are few and far between. Yet, group action is fundamental to political systems, and American public administration functions in the plural society. Holden concludes that the plural society and ethnic differentiation are origins of great turbulence in public administration.

In the epilogue, the author draws upon the interplay between continuity and disruption. He points out that the study of public administration becomes, in part, a study of how to sustain continuity and quell disruption. The study of public administration, states Holden, must leave its "self-assigned intellectual ghetto" (p. 251). Political science must recognize the realities of power in administering the law.

The epilogue proposes three themes. First, political science may profit from drawing more confidently on the work of our intellectual predecessors, grounded in an unrealized potential for a political theory of administration (chapters 1 and 2). Second, there is a need for new inquiry into the roles, uses, and limits of dogma as a means for crossing from theory and research into practice (chapters 3, 4, and 5). The reduction of turbulence in the intellectual environment may result in a neglect of disturbing facts. Third, there are major exogenous sources in administration that need further examination (chapters 6, 7, and 8). Technology effects and ethnic differences demand attention of scholars.

Norton E. Long (*The Polity*, 1962) stated that "the life blood of administration is power." Holden extends this point. "The life blood of power," he argues, "is administration" (p. 257).

Regardless of which word comes first, power or administration, Holden's insights are a valuable primer when confronting the upcoming turbulence in political science and public administration literature.

Media Entrepreneurs and the Media Enterprise in the U.S. Congress. By Karen Kedrowski. Cresskill, NJ: Hampton Press, 1996. 238p. $59.50 cloth, $22.95 paper.

Burdett A. Loomis, *University of Kansas*

Once, in commenting on the value of mavericks in the U.S. Senate, Sen. Joseph Biden remarked that one senator like William Proxmire was great for the institution, but thirty like him would be a disaster. Such an observation may be equally apt as applied to former Rep. Les Aspin (D-WI), perhaps the first "media entrepreneur" of the modern (post-1970) Congress. As noted by various journalists and scholars, including Karen Kedrowski in this work, Aspin demonstrated how a junior House member could employ a sophisticated understanding of the press to move issues onto the defense policy agenda.

Elected in 1969, Aspin schooled a generation of Democratic House members on how to move issues in a Congress confronted by a series of Republican presidents. Representatives such as Pat Schroeder (D-CO), Steven Solarz (D-NY), and Henry Waxman (D-CA) learned their lessons well, to say nothing of putting their own spins on obtaining national media attention for their policy concerns. House Republicans, most notably Rep. Newt Gingrich (R-GA), understood that media politics could be played effectively by minorities; Gingrich's rise to power in the House depended directly on his sophisticated ability to frame issues so that those beyond Capitol Hill could relate to them.

At the same time that a Gingrich, a Waxman, or a Sen. Phil Gramm (R-TX) sought press attention to advance issues (and their own careers), Aspin's legacy manifested itself in another and more malignant way: Legislators such as Rep. James Traficant (D-OH) and Rep. Robert Dornan (R-CA) increasingly used one-minute speeches in the House to mount colorful and occasionally vicious attacks on their opponents. Networks rewarded Traficant and Dornan with a few seconds of prime time, far more than almost any other representative, save party leaders, received. Like it or not, many House members have come to see the "media entrepreneur" role as appropriate, even valuable. This is the phenomenon that Kedrowski explores in *Media Entrepreneurs*.

Kedrowski's point of departure is that media entrepreneurs are different from other legislators in their dedication to and belief in the notion that obtaining publicity will allow them to set agendas, shape legislation, and even move a bill through the process. Although she draws upon work by Steven Hess and Tim Cook, Kedrowski is less skeptical than either Hess or Cook about the capacity of lawmakers to use the media to advance their legislative ideas. This creates some problems, in that she presents no systematic evidence that media entrepreneurs can actually accomplish their policy goals. The study relies on two complementary data sources: a survey of House and Senate offices and four case studies of issues that media entrepreneurs sought to shape.

The survey, with senior staff as the targeted respondents, obtained a modest (28%) response rate. Perhaps more troublesome is the nonspecific nature of the questionnaire's queries, which asks the staff to comment on the general usefulness of seeking media attention. Only a handful of items address the efforts of a specific legislator to solicit media coverage for policy purposes, and the responses are used sparingly in subsequent analysis. In addition, an appendix lists 74 interviews from Capitol Hill insiders, and information drawn from this source is used effectively in the text.

Who are the media entrepreneurs? This seems a core question, and Kedrowski spends a chapter offering some answers. Although she finds no clear trends in terms of demographics, she does see them as a distinct grouping within the Congress. But there's the rub, because the criteria for distinguishing media entrepreneurs are flawed. Kedrowski asked the top-level personal staff four questions about legislators' attitudes toward using media to set agendas and move legislation. She then combines the answers and creates a set of categories for individual members, based on their staffers' responses. Nothing in the questions related at all to their own members' behavior. Thus, the staffer for a relatively shy House member may believe that getting publicity is an effective way to operate, but such a conclusion may have nothing to do with her boss's attitude or behavior. In that a great deal of the analysis depends on this measurement of media entrepreneurship, further graded to distinguish "likely" and "hard-core" media entrepreneurs, many of the conclusions must be regarded with great caution. For example, Kedrowski finds no differences between senators and representatives, although she does note that the average senator gets a lot more coverage.

The linkage of media entrepreneurship to actual agendas, issues, or actions is missing, save for descriptions in the four case studies. So there is no analysis, either in actual or reported behavior, of whether the entrepreneurial style has an effect. Like many other scholars, Kedrowski dismisses the distinction between workhorses and showhorses, but she cannot offer any systematic evidence as to how media entrepreneurs succeed in agenda setting or shaping issues as they move through the legislative process.

Kedrowski does better in drawing upon her experience as a Congressional Fellow with Sen. Bob Kerrey (D-NE) and in developing four distinct, theoretically based case studies; she can report detailed stories about why legislators sought media attention and to what end. Indeed, she makes useful points in discussing when publicity is shunned, even when easily available. Likewise, her interviews with top-level journalists, while breaking no new ground, offer a different perspective on the actions of media entrepreneurs. What is clear is that both politicians and reporters understand that they use each other. Especially informative is her discussion of senators Jesse Helms (R-NC) and Alan Simpson (R-WY), both of whom rail at the press and effectively use the media, sometimes simultaneously.

In the end, this is a flawed but useful book. The quantitative analysis of media entrepreneurs can offer no definitive findings, but the qualitative descriptions and analyses provide real insights into the C-SPAN generation on Capitol Hill.

The Power of Separation: American Constitutionalism and the Myth of the Legislative Veto. By Jessica Korn. Princeton, NJ: Princeton University Press, 1996. 178p. $29.95.

Charles R. Shipan, *University of Iowa*

In the 1983 case of *INS* v. *Chadha*, the Supreme Court struck down as unconstitutional those legislative vetoes that violate the presentment and bicameralism clauses of the Constitution. Since then, a strong scholarly consensus has emerged regarding the demise of the extraconstitutional legislative veto. This consensus holds that the legislative veto was one of the most important ways in which Congress was able to respond to bureaucratic excesses and that the Court, by striking it down, deprived Congress of the ability to shape agency policymaking.

Jessica Korn forcefully takes on this conventional wisdom. Far from being a significant tool of congressional oversight, she argues, the legislative veto was an overrated, ineffective, and mostly symbolic measure. But Korn does not stop there. Instead, she goes so far as to counter the notion that the legislative veto at least improved policy responsiveness, arguing that its demise in fact had salutary effects on the workings of the legislative and executive branches.

While this reevaluation of the legislative veto is at the heart of Korn's analysis, it is only one layer of this beautifully written book. Korn embeds her examination of the legislative veto in much broader currents and arguments, most importantly in Woodrow Wilson's argument that our system of separation of powers is inherently unable to respond to the challenges of a rapidly growing, technologically advanced modern society. Korn uses the *Federalist Papers* to demonstrate that Wilson's analysis of the nature of the separation-of-powers system was incomplete. She points out that Publius, in order to overcome the objections of the antifederalists, needed to make a strong case for why such a system, with its checks and balances, would inhibit government action. At the same time, however, Publius articulated a distinct and more positive reason for creating a separation-of-powers system. This reason, which Publius considered as important as the protection of individual liberty, was that such a division of labor would foster specialization, which in turn would enhance the efficacy of the government.

Armed with this broader perspective, Korn turns her attention back to the legislative veto. What accounts for the disjunction between its actual and perceived power? Korn argues that a narrow focus on the legislative veto ignores the myriad of alternative means Congress has for influencing agency actions. From a Wilsonian perspective, the legislative veto was necessary to overcome the problems of separation. Yet, this perspective neglects the tools that the system allows Congress to use in order to ensure that agencies act appropriately.

In particular, Korn notes that informal versions of the veto, versions which were *not* ruled unconstitutional by the Court, supply Congress with immense power over agencies. Most notable among these are the "report-and-wait" provisions contained in many delegations of legislative authority, provisions that command agencies to inform Congress of their policy intentions before actually going ahead and implementing new rules. At the same time, notes Korn, the informal means at Congress's disposal are backed up by its ultimate ability to pass legislation that overturns an agency's action. Thus, informal powers force agencies to inform Congress of any prospective actions; and the power to write laws helps ensure that agencies will be sensitive to the desires of members of Congress.

Korn illustrates her arguments through a set of case studies that focus on the effect of legislative vetoes on the Federal Trade Commission (FTC), the Department of Education, and the president's ability to grant most-favored-nation status. Korn demonstrates that the legislative veto, far from being an effective oversight tool, actually had little practical effect on policy outcomes. Rather, she contends, it was at best a symbolic tool that allowed Congress to show it opposed overreaching by executive departments.

A good example of the limitations of the legislative veto's power can be found in Korn's analysis of the FTC. Widely perceived in the 1970s to be a runaway agency, the FTC seemed to be an ideal target for the legislative veto. However, as Korn demonstrates, the agency was perceived as being out of control not because of the rules it enacted but because it used its rulemaking power to announce so many *proposed* rules, causing business and industry to spend countless hours and millions of dollars on agency hearings. The legislative veto, which was aimed at preventing the passage of final rules, could not have directly affected these hearings. Congress, however, could and did use other procedural tools, such as the requirement for hybrid rulemaking and more easily triggered judicial review, to slow the agency down.

Is Korn convincing when she argues that the legislative veto was an ineffective tool and that it actually impeded Congress's ability to act properly? Not fully. At times she seems to push her arguments too far, conceding almost no power or positive effects to the legislative veto. She understates the extent to which the veto, despite its infrequent use, may have had a major effect on policy by causing agencies to alter their actions in anticipation of a veto. Might not the report-and-wait provisions have had more teeth because the agencies knew they were backed up by legislative vetoes? After all, a legislative veto was much easier to pass than a law; and it had much sharper teeth than the report-and-wait provisions by themselves. In addition, her argument that the demise of the legislative veto was beneficial in that it freed the House to pass unilaterally a symbolic measure criticizing China's human rights abuses is less convincing after she has derided the veto itself as being merely symbolic.

Yet, the forcefulness of her opinions no doubt derives from the strength of the consensus that Korn attacks. Her ability to provide a completely contrary interpretation, one that causes the reader to rethink previously held positions, demonstrates the power of her argument and her skillful use of case studies. These cases do generally support the notion that the legislative veto was not the dominant and positive force many thought it to be. Indeed, Korn could have supported her claim even further by pointing out the legislative veto's potential for producing biased outputs by shifting the distribution of power away from the entire Congress to a single committee or a single chamber.

Korn has produced a book that will be read with interest by a wide variety of scholars. Students of the legislative veto will find strong and generally convincing arguments as well as nicely detailed case studies. Those who study agency policymaking and those who study congressional influence on agencies will each find material to support their claims (although scholars who believe that Congress plays the dominant role in agency policymaking will come away more pleased). Finally, because of Korn's thoughtful analysis, students of the constitutional framework of our government will be forced to rethink the position that policymaking failure is inherent in our system.

Environmental Management and Governance. By Peter J. May, Raymond J. Burby, Neil J. Ericksen, John W. Handmer, Jennifer E. Dixon, Sarah Michaels, and D. Ingle Smith. New York: Routledge Press, 1996. 254p. $74.95 cloth, $24.95 paper.

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How local governments are used in environmental management has a significant effect on policy outcomes. Yet, surprisingly little social science has systematically addressed the role of local governments in environmental policies. This cooperative effort by a team of scholars from several different nations provides a substantial contribution toward closing the gap between the importance of local governments and the lack of scholarly analysis as to their effect. *Environmental Management and Governance* offers a compelling argument, an intriguing research design, candid empirical evaluations, and reasonable conclusions.

The argument is straightforward. These authors suggest that the way in which local governments are used in environmental programs will have significant consequences for policy outcomes. Put most simply, the authors differentiate between coercive and cooperative systems. In coercive systems, national and state governments prescribe regulatory actions and processes by local governments, monitor them for compliance, and enforce behavior toward national goals through sanctions. Cooperative systems use financial and technical assistance to encourage local governments to seek appropriate and innovative means to meet goals they share with higher-level governments. The authors then address the effects of such different systems on sustainable development, more specifically land-use decisions, especially in such hazard-prone areas as floodplains.

To assess the effects, the authors use an ambitious design to examine governments in three different nations that vary widely in terms of coercion or cooperation. The coercive approach is illustrated by Florida's efforts to promote local planning and control in coastal areas. New Zealand's cooperative approach since the late 1980s involves local governments in a partnership with the national government to plan and zone for floodplains and other areas. Flood policy in the Australian state of New South Wales has shifted in recent years from relatively coercive to much more cooperative. The use of such different settings is necessary, the authors argue, to examine the wide range of possible approaches. This comparative aspect of the project also explains why seven different authors from four nations are involved. Such a design may seem overwhelming, but I admire the scope of the project and applaud the willingness to examine related phenomena in such potentially different contexts.

While the authors state clearly that their major interest is to examine whether cooperative approaches overcome the limitations of more traditional coercive efforts, they are admirably candid in assessing the empirical evidence and avoiding normative judgments. The evidence ranges from narrative historical accounts to statistical appraisals of employee surveys. The analyses reveal some expected and some unexpected findings. Both systems face implementation problems, but some of these, such as uniformity of delivery, are more difficult with cooperative programs. Local compliance is greater under coercive mandates, but local innovation is higher in cooperative systems. The flexibility to be gained from using cooperative efforts is, to some degree, countered by the variability in commitment from different local governments.

The authors quite reasonably conclude that neither style is ideal and that both have limitations. Furthermore, to their credit, the authors do offer some recommendations, particularly about the facilitating conditions under which the virtues of a cooperative system may be realized. Finally, the authors suggest, albeit succinctly, how lessons from these programs may be generalized to other policies in other settings.

A project this ambitious and innovative is bound to inspire questions. The most obvious, comparability of cross-national cases, is one which the authors anticipate early and meet head on. Defending even the comparison of the State of Florida to the nation of New Zealand, they argue that "the similarities ... far outweigh the differences" (p. 11). I did not find this as troubling as some comparative scholars may, but some aspects of such comparisons warrant more discussion. One of these, mentioned on the last page of the Appendix, is that much greater growth pressures in Florida than in New Zealand or New South Wales could well lead to a much different environment for local governments. A second and related aspect involves competitive influences such as relocation blackmail. The authors themselves cite a New Zealand minister's concerns about lowering standards to attract development (p. 57). The effect of such pressure on state and local governments worried about losing business to the next county or state could receive fuller consideration.

A second question concerns focus. The book examines the effect of system type on policy outcome but is much less concerned about the front end of those arrangements: Why are coercive or cooperative styles chosen in the first place? This is understandable given the emphasis of the project and is probably necessary for purposes of length, but more comprehensive answers about the choice of institutional arrangements may help inform the answers to why certain outcomes result. For example, referring back to competition pressures, Florida may consider a coercive style the only reasonable option for how to use local governments in the face of competition from Georgia or Alabama over coastal developers.

A third issue involves possible connections between this project and the broader literature on intergovernmental relations. Though the authors' citations (and previous work) indicate an understanding and awareness of that literature, I personally would have preferred to see deeper and more pointed reflection on how the results of this project extend, contradict, or expand upon some of the research in this field. Having started this review by stating that the role of local governments has not received enough attention, I must also say that it has received some, and some of it is quite good. I would have liked to see, for example, fuller consideration of the theoretical implications of this project for the arguments posed by Paul Peterson and others about the incentives of policymakers in local governments.

Finally, a lesser but related point is more stylistic. Readers should be aware that the book uses endnotes for each chapter but does not include a bibliography. Perhaps the reason—the wide range of literature needed for the scope of the project—is the same reason that some readers would find one desirable.

Overall, this is an impressive work. It presents challenging ideas as examined in a variety of settings and does so in a clear and readable manner. This book will be useful for policy practitioners as well as for classes on environmental policy, intergovernmental relations, and even comparative politics.

Education and Democratic Citizenship in America. By Norman H. Nie, Jane Junn, and Kenneth Stehlik-Barry. Chicago: University of Chicago Press, 1996. 268p. $48.00 cloth, $16.95 paper.

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The presumed importance of formal education to good citizenship has been deeply imbedded in the theory and practice of democracy in America since the founding. Education provides the skills and knowledge for creating a productive, informed, and engaged citizenry. It is the great equalizer that helps level the economic, social, and political playing field.

Survey research since the 1950s has provided consistent evidence of the value of education at the individual level. Indeed, in his 1972 essay, "Change in the American Electorate" (in Angus Campbell and Philip E. Converse, eds., *The Human Meaning of Social Change*), Philip E. Converse describes education as "the universal solvent," strongly and positively correlated with a host of valued civic attitudes and behaviors.

But there is a problem. Despite the steady increase in the average years of formal education attained by Americans, despite the shrinking gap in education among citizens, and despite the strong connection between *individual* measures of good citizenship and education, aggregate indicators of civic involvement show little evidence of increasing, and in many instances they have declined over the past several decades. It is this apparent paradox that Norman H. Nie, Jane Junn, and Kenneth Stehlik-Barry attempt to resolve in their excellent book, *Education and Democratic Citizenship in America.*

The authors begin by arguing that, at the individual level, education enhances democratic citizenship through two distinct pathways. "Democratic enlightenment," defined as an adherence to democratic norms and the recognition of collective interests that can contradict and override one's individual preferences, is centrally affected by education's ability to increase the cognitive capacity of citizens. This in turn gives people the motivation and tools to appreciate both the logic of democratic norms and the potential legitimacy of views different from their own. "Democratic engagement," or the ability to understand one's own interests and effectively pursue them through the political system, is largely connected to education through one's social position and the resulting access to centers of political power and decision making.

In Part I (chapters 2 through 5), the authors use data from the 1990 Citizen Participation Study to demonstrate that civic enlightenment (measured as political tolerance, knowledge of democratic principles, and knowledge of current political facts) and civic engagement (measured as voting, political attentiveness, knowledge of political leaders, and participation in difficult political activities) are two correlated yet distinct dimensions of citizenship; that formal education is positively correlated with each dimension; and, most important, that the effect of education on civic enlightenment occurs largely through the intervening variable of "cognitive proficiency" (measured by a verbal proficiency test), while the influence of education on civic engagement occurs through a more complex set of pathways dominated by "social network centrality" (measured by respondents' self-reports of how personally acquainted they are with local, state, and national elected officials and with local and national members of the media).

In Part II the authors draw on this theoretical argument and empirical evidence to address directly the paradox mentioned above: If education is positively correlated with civic engagement at the individual level, and if median levels of education have increased dramatically in the United States over the past several decades, then why have median levels of civic engagement not increased? Other researchers have noted this paradox but have largely addressed it by assuming that other societal changes (the weakening of political parties, the erosion of civil society, the increased complexity of politics, the declining quality of education, the growing dominance of television as a source of political information, and so forth) have worked to cancel out the positive effects of education.

Nie, Junn, and Stehlik-Barry take a different tack. Building on the logic of Fred Hirsch's *Social Limits to Growth* (an important and, to my mind, undercited book), they argue that education's collective effect on civic enlightenment, since it is driven mainly by individual cognitive proficiency, is absolute, while education's collective influence on civic engagement, because it is driven by one's location in a sociopolitical hierarchy, is relative and thus bounded. The distinction is an important one. Most theory and research on the effect of education assumes the absolute model: If attributes of good citizenship increase with education, then raising the median level of education should increase the median level of good citizenship. But if the influence of education is relative, then greater aggregate levels of education are equivalent to running in place; a college degree today gets you what a high school degree did in the past.

The authors test their theory using pooled data from the General Social Surveys and from the National Election Surveys from 1972 to 1994. Their models include measures of *both* absolute years of educational attainment *and* the relative educational environment (measured as the mean years of education attained by *all* respondents aged 25 to 50 years at the time the respondent was age 25), as well as other variables to control for the effects of life cycle, generation, demographic characteristics, partisanship, and family and community characteristics. They also include dummy variables for the year of each survey so as to de-trend the data and isolate (as best as possible) the effects of education from other exogenous factors not included in the model.

The authors find that, consistent with their theoretical expectations and the findings from Part I, civic enlightenment increases with both absolute education and the relative educational environment, while civic engagement is positively correlated with absolute education *but negatively correlated with the relative educational environment.* They then use the results of these analyses to estimate the predicted mean level of enlightenment and engagement over time, and they find a remarkably strong fit between the predicted and actual trends from 1972 to 1994. In short, they find that the absolute and relative effects of education work cumulatively to increase civic enlightenment, while the positive effects of absolute levels of education on civic engagement are neutralized and sometimes overwhelmed by the decreasing relative value of education (as a social sorting mechanism) over time.

The authors' conclusions are sobering. While the absolute effects of education theoretically may increase citizens' ability to be engaged, relatively fixed limits on the ability of political elites to respond to demands mean that the political system will always be most responsive to those who, because of their social status, are best positioned to be heard. Given these unknown but bounded limits, increased education simply leads to more fine-grained decision rules as to who sits at the political table. More education is necessary for individuals, but only to hold one's place in the sociopolitical hierarchy as others become more educated. Efforts to alleviate social inequality through increased education, while well intentioned, will inevitably lead to conflict, since political access for new groups and individuals necessarily means less access for others. Given this, the authors end by suggesting that the social, political, and economic costs of policies aimed at increasing the aggregate level of education in the United States *may* outweigh the benefits provided.

One can take issue with parts of the theoretical arguments and empirical analyses presented in this book. Why, for example, would cognitive proficiency logically increase one's ability to appreciate the legitimacy of the interests of fellow citizens but not increase the ability to discern one's own interests? Why should knowledge of current political facts (such as which party controls Congress) be considered an attribute of both civic engagement and civic enlightenment, while knowledge of political leaders be conceptualized as an attribute of only civic engagement? Why do voting and political attentiveness, which the authors hypothesize should be part of both civic enlightenment and civic engagement, appear to load only on the latter dimension. This last point is especially troubling, in that voting and attentiveness do not clearly fit the logic of social network centrality, which assumes limited access to those in power and politics as a zero-sum game.

More generally, the authors' vision of politics as a zero-sum game played by individual actors in contact with individual political leaders who control limited, divisible goods and services misses the more collective, consensual aspects of politics. And even if one accepts the fundamentally competitive nature of politics, thinking in terms of competition among groups rather than individuals lessens the sense of system overload and changes the calculation regarding the value of a more educated—and more equitably educated—citizenry.

Nonetheless, taken as a whole, *Education and Democratic Citizenship in America* presents an elegant, challenging, and innovative argument that is strongly supported by the data. The authors provide important contributions to the way in which we conceptualize citizenship and the way in which we conceptualize the civic effect of education. This book is a must read for anyone interested in issues of democratic politics and will have a major influence on the field for years to come. Quite simply, it will be impossible to consider the effects of education on citizenship, political participation, and public opinion without addressing and building on the theories and findings presented in this book.

Who Deliberates? Mass Media in Modern Democracy. By Benjamin I. Page. Chicago: University of Chicago Press, 1996. 167p. $29.00 cloth, $10.95 paper.

James S. Fishkin, *University of Texas at Austin*

Ben Page's new book has many virtues. It is readable, thoughtful, and built around some case studies that will engage anyone interested in politics. The book is also more than a series of essays because Page ties them together by posing a common problem and reflecting on what his case studies mean. The common problem is one that cuts to the core of democratic theory. "A vigorous democracy cannot settle for a passive citizenry that merely chooses leaders and then forgets entirely about politics" Page says. "Some kind of effective *public* deliberation is required that involves the citizenry as a whole" (p. 4, emphasis in original).

Can effective public deliberation be achieved? In Page's view, we are much closer than most critics of the press and public opinion assert. The answer is relatively straightforward. "If extensive political information is available somewhere in the system, not everyone has to pay attention to it all the time." The reason is that information "will trickle out through opinion leaders and cue givers to ordinary citizens, who can deliberate about it in their own small, face-to-face groups of family, friends and co-workers" (p. 7).

Yet, Page offers little discussion of how deliberation may or may not work among ordinary citizens, among family and friends in small face-to-face groups. Instead, the book quickly narrows its focus to a single answer to the question posed by the title: "Who Deliberates?" The answer is in the subtitle "the mass media in modern democracy."

What does Page mean by "deliberation"? He means simply "reasoning and discussion about the merits of public policy" (p. 2). While there is now a great deal of historical, theoretical, and empirical work that attempts to narrow the definition, to set up more demanding criteria for "deliberation" as opposed to just any "talk" about policy or politics, Page generally ignores such issues. Hence, there is little concern for whether argument is met by counterargument, for whether the public becomes informed about the competing arguments, for whether the public becomes more consistent or even attentive to the way that its values are connected to the competing policy options. Instead, all we need for deliberation is that there be reasoning and discussion about the merits of public policy by someone somewhere in the system—accurate or not, balanced or not, informed or not, widely participated in or not, connected to the values or concerns of the public or not.

I have two concerns about Page's approach to public deliberation: whether it is "deliberation" and how it is "public."

The first is capsulized by one of Page's examples. When Bush's Press Secretary, Marlin Fitzwater, mentions in an off-hand comment that the root causes of the Los Angeles riots might be found in failed social programs of the 1960s, Page hails discussions of this comment on the network evening news and on CNN the same day as "illustrating the amazing speed with which deliberation can proceed through electronic media" (p. 47). Perhaps. But the discussion on "Crossfire" which Page heralds was so stereotyped, so posed for effect, and so misleading in its content that I am skeptical about whether it should be lauded as an achievement of American democracy. Media pundits on CNN give us an "inside the beltway" version of staged conflict—professional mud wrestling rather than genuine drama. I think there is an argument for reserving such lofty terms as "public deliberation" for something more substantive, balanced, and informed.

And where is the public in this "public deliberation"? Citizens in this book are mostly just viewers, passive spectators who are largely inattentive. This is deliberation *in* public but not *by* the public, despite Page's references early on to "face to face discussion" by original citizens and to the notion that "effective public deliberation" must involve the "citizenry as a whole." One would not know from *Who Deliberates?* that there are literally thousands of civic groups around the country attempting to nurture or stimulate "public deliberation" by the public, not just in front of the public. Recent efforts to bring the public into the deliberative process, by the National Issues Forums, citizen juries, and civic or public journalism, all go undiscussed.

The public does, however, break through in one of Page's major cases, but here it is doubtful that when we finally get to the public, rather than to the pundits, there is any real "deliberation." The case is the uproar formented by talk radio about Zoe Baird's "nanny" during her contested confirmation hearings (one of Page's cases of public deliberation). An affecting sound bite led to thousands of listeners feeling sound-bitten. They responded with enraged calls and faxes to members of Congress. This may indeed have been public, but was it deliberation? Ordinary citizens knew almost noting about the merits of Baird's confirmation case except that she had not paid social security taxes on a nanny. The real merits and their policy implications did not break through the barriers to pubic inattention, even though a highly distorted, selective account of the merits of her case did. This is not "deliberation" worthy of the name—it is not deliberation that, say, Madison, would recognize. Rather, it is the "passion" of the public that the founders would have feared.

This book is part of a research program which Page introduced earlier with his impressive collaboration with Robert Y. Shapiro (*The Rational Public*, 1992). It is a research program that offers many insights but that also presents a picture of public opinion which has proven controversial. It declares that the system of public discussion and opinion formation is really working much better than most academic and media critics believe.

As Page states in his conclusion to this book, "the finding that there is a rational public with generally sensible and responsive policy preferences, indicates that enough diversity and competition among ideas is provided so that mediated deliberation usually works fairly well" (p. 123). Page is telling us that the public is rational and the system is providing ample opportunities for deliberation. The media provide the missing link in explaining how "deliberation" occurs for the benefit of a "rational" mass public. There are many of us, however, who believe that the public is often less than rational. Rather, it is often only "rationally ignorant," inattentive, and uninformed. Furthermore, the media is doing far less to serve the democratic dialogue than it could. For those of us who take this position *Who Deliberates?* will offer little comfort. We can ask the question posed by the book's title and still worry about whether we have found an answer.

Building a Democratic Order: Reshaping American Liberalism in the 1930s and 1940s. By David Plotke. New York: Cambridge University Press, 1996. 388p. $49.95.

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David Plotke has undertaken an important task here. His book attempts nothing less than the reintegration of state theory with earlier portraits of parties and electoral systems. The result is a challenging reconsideration of the transformations of the 1930s and 1940s that "brings parties back in," if only through the back door.

Plotke's primary focus is on the construction and maintenance of the political regime of the 1930s. By this he means the tripartite supportive relationship that came to exist between the Democratic Party, the national state, and the regime's newly established interest-group coalition. The focus here is never simply on mass electoral behavior, state capacity, or the consideration of separate struggles for reform. Plotke is concerned with tracing the broader patterns of party–interest group interaction that rapidly transformed both American state capacity and the broader political order.

The organizational scheme of the book is clear and straightforward. Plotke applies a four-point structure to account for the rise and ultimate decline of the "Democratic state." The "regime" concept remains the focus of most of the chapters and the models built around these four organizing principles. The first, "Building a Political Order," emphasizes the initial emergence of new political forces that attacked the old order and mobilized support for new policy initiatives. This process is accompanied by high levels of political mobilization that create new political and organizational identities and forms. The second phase, "Consolidation and Maintenance," involves new party-state relations that place less emphasis on popular mobilization while creating a greater political role for state structures. Phase three returns to more state-centered models by placing more reliance on the activities of the governmental structure in sustaining the regime's political and organizational interests. In contrast to the previous levels of mobilization, the state now becomes the central piece sustaining the regime's organizational network. Phase four, "Decay and Decline," points toward the long-term decline of nonstate modes of political support, largely due to the persistent reliance on the state's now dominant place in organization and regime maintenance. The inability to accommodate newly emergent forces successfully becomes chronic, and the overall legitimacy of the regime is persistently challenged.

Plotke maintains that the rise and, equally important, the subsistence of American liberalism after 1933 is best understood through this tripartite model. By this view, the Democratic electoral successes of the 1930s pointed toward policy change in areas such as social welfare or economic regulation that would create institutional and constituency support. Both would be central to the successful consummation of the 1930s realignment. As a result, much of Plotke's analysis centers on important policy initiatives such as the Wagner Act, which while reflecting immediate political concerns also created important clientalistic groupings that entered into alliances with state power.

Yet, nagging questions remain underconsidered by the narrative. If the combination of electoral and state-centered changes was so sweeping in the 1930s, why did non–New Deal culture remain so persistent? Plotke is well aware of the staying power of the old antistatist ideology, but his discussion moves back and forth between descriptions of efforts to discredit these belief patterns and his own recognition that these forces remain such a central feature of American political life. His treatment of the domestic New Deal is disproportionately centered on the 1935–36 period, when structural reform reached its highwater mark. Curiously, the 1938 congressional elections are almost completely ignored. Yet, it is here that limits to the ideological transformations of the period were first suggested.

The material on the Hoover administration reads too much like the descriptions composed in the 1950s prior to the opening of archival material. So, too, does the Depression analysis, which contains little that is fresh or new. The author has done a careful, thoughtful reconstruction of the kind of business interests that came to oppose the Wagner Act; he is far less clear as to why this opposition remained so persistent or pronounced throughout the period. At points Plotke falls into the trap of explaining opposition by referring to the political culture of business and in general the conservatism of American society. An enhanced understanding of the complex sources of non–New Deal culture would have strengthened the narrative at critical points. There is a long-term tendency for structural analysis to focus primarily on the role of the South in the old Democratic Party and Congress that obscures and minimizes the broader patterns of ideological opposition that made the "Democratic state" far more reactive and defensive by the late 1940s. At points some of the material presented in this book reflects this.

The 1936 Roosevelt-Landon election receives sustained treatment, as it so clearly demonstrated the organizational strength of many of the newly emerging groupings described by Plotke. The book then carries the evolution of the "Democratic state" through the Truman period, the early civil rights conflicts, and even the Cold War. Most of the material is presented in a fashion that provides an excellent overview of the convergence of modern liberalism and the state after 1932.

Plotke has deepened our understanding of the multifaceted elements of the "Democratic state" that had emerged by 1936. The work's great strength lies in its effective, careful application of differing theoretical approaches as they apply to regime establishment and maintenance. It is a work of insight and integrative power in a field increasingly characterized by compartmentalized approaches and interpretations.

The New Citizenship: Unconventional Politics, Activism, and Service. By Craig Rimmerman. Boulder, CO: Westview Press, 1997. 154p. $45.00 cloth, $15.95 paper.

Dick Simpson, *University of Illinois at Chicago*

The New Citizenship is ideal for use as a supplement for American government courses or as a text in courses on elections, political participation, community organizing, or community service. It seeks to answer the empirical question, "What role does the citizenry play in the American political system?" and the more philosophical question, "What role should the citizenry play?" (p. 2). In this it is similar to earlier books, such as Lester Milbrath's *Political Participation* (1965) and Wolfinger and Rosenstone's *Who Votes?* (1980).

As with many political theorists, Rimmerman distinguishes between the democratic theory of elitism, "which favors a limited role for the citizenry, one where citizens participate in periodic elections and elect well-educated and well-trained elites to represent them in the public policy process," and participatory democracy, which depends upon "increased citizen participation in community and workplace decision-making" (p. 6).

Rimmerman cites low voter turnout in both presidential and off-year elections, the number of incumbents who voluntarily leave office, the level of citizen alienation, and the especially low level of participation by college students. He differs from many accounts of current politics by also focusing on the civil rights movement and the foundation it provides for "New Citizenship," while at the same time it establishes "a foundation for disruptive politics and potential threats to system stability" (p. 62). One of the distinguishing aspects of the book is its account not only of the civil rights movement but also of modern movements of unconventional politics, such as ACT UP, Earth First!, Operation Rescue, the militias, and neighborhood and student organizations.

A particularly positive feature of the book is its advocacy of what the author calls "New Citizenship," which goes beyond merely voting to include grass-roots mobilization, community participation, service learning, and the internet. Part of his argument is that "citizen organizations have expanded and increased participation in American politics at the same time that many Americans eschew voting" (p. 95). One way to extend "New Citizenship" more broadly is to provide a "critical education for citizenship" to "enable students to define what they mean by democracy and . . . to develop necessary skills for participation on their campuses, in their communities, and in the larger society" (p. 100). This program of citizen education includes community service courses and internships as a part of the curriculum.

As a text, *The New Citizenship* differs from most political science alternatives. It bridges between empirical social science data and theories and citizens' broader experiences in society. For the development of an alternative political philosophy, it is not nearly as solid a contribution as Carole Pateman's *Participation and Democratic Theory* (1970) or Jane Mansbridge's *Beyond Adversary Democracy* (1980), whom the author cites. No does it provide as good an explanation of representative democratic theory as Hanna Pitkins *The Concept of Representation* (1967). Yet, the author does confront the difficult problem of current low levels of voter participation occurring at the same time as higher levels of unconventional political activism and alienation. This makes the book worthwhile for scholars and a good adoption as a textbook for students.

Thus, while *The Citizenship* may not completely change the dominant view of our discipline on representative democracy, citizenship, or college curriculum, it begins to provide a practical alternative to the democratic theory of elitism and acceptance of a nonparticipatory status-quo politics. It is a building block in a larger intellectual project and a helpful first step in making political science courses relevant to our students once again. It is definitely worth considering for classroom adoption.

A Matter of Interpretation: Federal Courts and the Law. By Antonin Scalia. Princeton: Princeton University Press, 1997. 159p. $19.95.

David Schultz, *University of Wisconsin, River Falls*

Reconciling judicial power and discretion with majority rule is one of the most enduring and perplexing dilemmas in American politics. From the time Alexander Hamilton penned *Federalist* 78, indicating that the power to interpret the law would not render the Supreme Court a threat to Congress or the President because it would be the "least dangerous" branch, to Alexander Bickel arguing in *The Least Dangerous Branch* (1962) that judicial power raised the specter of countermajoritarianism, efforts have been sought to limit the power of the Court and justices to substitute their own views in place of those held by legislatures elected by political majorities. For Hamilton, the solution to constraining judicial power was to ensure that the courts have "neither FORCE nor WILL, but merely judgment" that would be constrained by strict rules and precedents. For Bickel, judicial tendencies to engage in countermajoritarian action could be constrained by requiring the Court to base its decisions upon neutral principles of law.

In between Hamilton and Bickel others, including Ed Meese and Robert Bork, have engaged the debate on how judges ought to interpret common, statutory, or constitutional law, with the assumption being the neutral rules of construction or principles of law would constrain judicial discretion and define the proper limits of the courts within American democracy.

Continuing in this concern for drawing the proper limits to judicial power, Justice Antonin Scalia argues in his most recent book that reconciling judicial statutory and constitutional interpretation with democracy and common law is preeminently a problem that can be solved by judicial adherence to neutral rules of interpretation. Throughout his judicial and scholarly career, Scalia has sought ways to define the limits of judicial discretion, invoking various and ostensibly self-described neutral tools of interpretation. In rendering his views, the justice often finds himself at odds with the rest of the Court, claiming, often in acerbic tones, that the majority has overstepped its judicial bounds.

For Scalia, the dilemma confronting the judiciary is one of reconciling the power of common law judges to interpret and often make the law with the demands of a democracy and separation of powers that ostensibly place limits on judicial power. If Scalia is correct that common law interpretation runs afoul of the problem of *ex post facto* law, then how do we assure that judges are not simply substituting their personal judgments on the law in place of that determined by legislatures? The justice's answer resides in what he calls the "science" of interpretation.

Drawing upon claims he has articulated in earlier essays and his opinions, Scalia provides what appears to him to be a simple solution—force judges to follow rules of construction. Reading like a script out of a first-year law school curriculum, Scalia tells us to treat the text seriously, follow canons and presumptions of interpretation, and eschew legislative history and intent as unreliable ways to read laws. When he turns to the Constitution, Scalia asks judges to look to the intent of the framers and turn away from adopting a living and flexible view of the Constitution. Overall, Scalia's solutions are meant to "anchor" judicial interpretations in something other than a judge's own predilections.

Scalia's opening and closing essays represent only part of this slim volume. Engaging the justice are fascinating essays by Gordon Wood, Laurence Tribe, Mary Ann Glendon, and Ronald Dworkin. Wood, for example, offers solid evidence that from the early days of the American republic judges were viewed as important checks upon legislative behavior, thereby compromising the strict separation of powers and judicial/legislative antinomy that Scalia sets up. In addition, Wood offers perhaps the most cogent criticism of Scalia, noting that it is not the techniques of interpretation that constrain judges but, instead, the attitude and sense of judicial role that judges bring to the bench.

Besides Wood, comments from Dworkin and Tribe rehash their earlier claims about constitutional interpretation, problems in ascertaining original intent, and pleas to view the Constitution in terms of broader principles or aspirations by the framers. Finally, Glendon brings a comparative perspective to interpretation, contrasting how code-based and common law systems address judicial interpretation. Glendon's essay is potentially the most informative yet the least developed.

Much of what Scalia has to say is disappointing. His two short essays do not go much farther than the aphorisms of his found in earlier cases and articles, and his solution to resolving the problem of judicial power in a democracy by following rules of construction is both simplistic and prone to abuse, as Scalia's own judicial record demonstrates. Merely forcing judges to follow rules of construction will neither constrain behavior nor ensure "correct" results. In addition to the problems existing with the "open texture of language," there is good evidence that justices can use any of several different interpretation techniques to reach the same result. The multiple interpretative techniques in *Dred Scott* v. *Sanford* show this, as well as the interchangeable uses of various techniques of interpretation employed by both Brennan and Rehnquist to support their holdings.

Moreover, Scalia's own decision-making pattern fails to support his own theory of interpretation. As Chris Smith and I argued in *The Jurisprudential Vision of Justice Antonin Scalia* (1996), Scalia's self-described neutral rules of interpretation are not so neutral. Instead, they are guided by a political philosophy demonstrating clear policy commitments that question the efficacy of his own rules of construction in constraining his behavior.

Finally, Scalia's formalistic and rigid view of courts versus legislatures is driven by a view of separation of powers accepted on the Supreme Court by perhaps only himself and Clarence Thomas and probably not even endorsed by the framers after the adoption of the Bills of Rights. By that, while the Federalist view of the Constitution certainly embraced separation of powers and a congressionally dominant mode of protecting rights, adoption of the Bill of Rights shifted the focus of political power and rights protection to the courts. As Justice Jackson aptly stated in *West Virginia* v. *Barnette* (319 U.S. 624 [1943]), "the very purpose of a Bill of Rights was to withdraw certain subjects from the vicissitudes of political controversy, to place them beyond the reach of majorities and officials and to establish them as legal principles to be applied by the courts." Scalia's vision of American politics, judicial power, and his techniques of interpretation seem to miss the profundity of Jackson's statement, as well as the role the Bill of Rights placed upon judges and the courts.

The Making of an American Senate: Reconstitutive Change in Congress, 1787–1841. By Elaine K. Swift. Ann Arbor: University of Michigan Press, 1996. 248p. $39.50.

Richard Bensel, *Cornell University*

By way of this fine book, Elaine Swift has given us a highly sophisticated account of the transformation of the U.S. Senate from the founding through the Jacksonian period. Members of the Constitutional Convention attempted to transplant on the soil of the New World one of the most hallowed products of the Old World, the House of Lords. In deliberations more prolonged and complex than those that attended the creation of any other branch of the new American regime, the founders struggled over questions associated with apportionment, powers, terms, and selection. In settling on the House of Lords as a model, they sought to establish a stolid and reliable counterweight to the more popular, and thus unreliably erratic, House of Representatives.

For two decades after it first met in 1789, the Senate remained faithful to this model. As one of the primary pillars of the new regime, senators maintained a sober independence from their ostensible masters in the state legislatures and an even more respectable distance from the citizenry. As the upper chamber of a bicameral Congress, the Senate maintained a passive stance toward the House of Representatives with regard to the initiation of legislation but was quick to reject or radically amend measures sent up from that branch. And as one of the several branches of a federal system in which a separation of powers was meant to underwrite institutional independence and competition, the Senate behaved toward the president in much the same way that the House of Lords showered solicitude and loyalty on the king. In sum, the new Senate in these early years functioned as it was designed: a deeply conservative institution, sensitive to the concerns of the economic and social elite of the new nation.

Over the next thirty-two years, however, from roughly 1809 to 1841, the Senate reversed field. A small but highly influential core of members transformed the institution in at least three ways: by strengthening ties with the citizenry, almost making an end run around the state legislatures in an attempt to build a popular base from which to compete with an ever more aggressive House of Representatives; by engaging the House along the entire spectrum of legislative deliberations, from initial drafting through oversight; and by establishing a more neutral and distant posture with respect to the executive branch. This transformation produced many of the characteristics of the modern Senate, the "strange mix of marked elitism, considerable autonomy, and sweeping power" (p. 2) that so often seems at odds with other national political institutions and trends. The same reconstitution of internal design and external relations saved the Senate from the contemporary irrelevance and anachronistic decline that has befallen the House of Lords.

The radical reorientation of the Senate's role in the national political system before the mid-nineteenth century was the product, in the author's terms, of "reconstitutive" change: "a rapid, marked, and enduring shift in the fundamental dimensions of the institution" (p. 5). Such change can only result from a conjunction of interrelated factors, some of which are external to the institution (e.g., the emergence of mass political parties) and others which are internal (e.g., the pursuit of an "institutional vision" by a vanguard of activist members). The theoretical lesson to be learned is that such "reconstitutive" change exhibits, albeit rarely in history, a punctuated rhythm in sharp contrast to the long-term, secular trends associated with Nelson Polsby's theory of institutionalization.

The political lesson to be learned is that effective institutional reform cannot take place in the absence of a comprehensive vision of an institution's place within the national political system, a vision shared and pursued by the members themselves. Those who would strengthen Congress vis-à-vis the presidency in the late twentieth century would do well to study how senators remade their institution 150 years ago. In this sense, the high theory and subtle exploitation of political possibility that allowed them to reconceptualize the place of the Senate within the panoply of American institutions and governments is a model for contemporary reform.

While the author's claims for the contemporary relevance of institutional change in the early nineteenth century should be seriously heard, they are not the only context in which her work is a significant addition to the literature on Congress and American political development generally. There is much in this book that suggests it would be even more useful to view the early Senate as part of a new nation and infant political regime in which the quest for popular legitimacy led practical reformers to emphasize continuity with prerevolutionary institutions. In such a context, the construction of a Senate that mimicked the House of Lords, installed beside a House of Representatives structured along the lines of a House of Commons and a presidency that possessed some of the trappings of a monarchy, made good political sense. In some ways, also, the revolutionary nature of the new regime made the Senate the institutionalized home of the "junta" that led the American Revolution, along with a president who was merely first among that number. This interpretation fits with the composition of the first Senate and its cooperative relationship with Washington (as anticipated in the founders' institutional design). In addition, the more divisive aspects of the Revolution (e.g., the massive flight of loyalists to Canada) can be incorporated in such a way that this new regime does not appear quite as secure as the present work at times seems to imply. Once this new regime no longer feared popular rejection, truly innovative institutional designs could be entertained. That is the point at which the Senate activists of the early nineteenth century found their opportunity. Seen this way, the lesson to be learned is very timely indeed; but, in this case, it is a comparative lesson for all the fledgling democracies of the East.

Comparative Politics

City of Stone: The Hidden History of Jerusalem. By Meron Benvenisti. Berkeley: University of California Press, 1996. 274p. $24.95.

Governing Jerusalem: Again on the World's Agenda. By Ira Sharkansky. Detroit: Wayne State University Press, 1996. 245p. $36.95.

Hillel Frisch, *Hebrew University of Jerusalem*

Jerusalem, whose name ironically may be linked etymologically to peace, has in the past 150 years become synonymous with conflict. In fact, the recent history of this city is a microreflection of the promise of modernity yet at the same time a poignant reminder that the progress, reflected in its transformation from medieval-like squalor at the beginning of the nineteenth century into a relatively prosperous urban center (at least regarding Jewish west Jerusalem), has coincided with national and religious conflict. The pronouncement in the wake of the Oslo Accords made by Faysal Husayni, the designated mayor of the would-be capital of Palestine, that the battle over Jerusalem between Israelis and Palestinians had begun is probably correct if emended to mean the beginning of the latest stage of a much longer conflict between the two protagonists.

This widely shared perception that yet another stage of the battle has begun, if not from the Declaration of Principles Agreement of 1993, then at least from the intifada that broke out at the end of 1987, may explain the spate of scholarly and more popular books that have appeared on Jerusalem in the past several years. The two under review are the latest and arguably the best of this generally high-level crop.

Sharkansky's work, in focusing on city governance and its relationship to urban and national policy formulation and practice, is both strictly academic and written in the political science tradition. In studying one example of a generic category of deeply divided cities which possibly deserves comparative treatment, he begins by explaining why Jerusalem is nevertheless distinctive and should be treated as a unique case study. For example, Sharkansky dismisses two of the closest parallels to Jerusalem, Belfast and Nicosia, because they lack the holy sites with which Jerusalem is both blessed and cursed; they thus find it difficult to attract foreign resources either to develop their respective cities or to attenuate conflict in order to preserve them. Jerusalem is an outstanding example of how rivalry and conflict need not necessarily be invidious to progress, though one can make a case that prosperity would be even greater in its absence. But these cities do share many salient characteristics: home to two or more competing religions and nationalisms, run by states with which the majority of the city population identifies and which the competing minority vociferously opposes, and with a community that can and does appeal to a variety of extraterritorial forces, ranging from external blocs of states to powerful diasporas, to bolster its hold in the city. In all three cities, violence has erupted between the communities, between the state and the opposing community, between the state and extremists within the supportive community, and between states.

Sharkansky's fine analysis would have been enriched by referring to the comparative literature and doubly so by using his vast knowledge to contribute to theorizing on the governance of deeply divided cities. Israelis and Palestinians will need all the help they can get constructing a political division of labor that will ensure the overall level of prosperity and stability that Israel's sole rule over Jerusalem has achieved, even though its Arab inhabitants, as both Sharkansky and Benvenisti emphasize, have suffered from unequal treatment and allocation of resources. Sharkansky's discussion concerning whether policies of domination or accommodation contribute to stability and economic welfare could be a good starting point for more comparative work on the governance of deeply divided cities.

This lack in terms of comparability is offset by the powerful historical parallels and distinctions Sharkansky draws between the situation of the Jews in Jerusalem and in Israel generally today as opposed to the biblical and Second Temple periods. The work is also strong in addressing the issues of governing Jewish Jerusalem, hardly a simple exercise given its distinctive subgroups, including the academic-professional secular (who increasingly are fleeing the city either to a nearby suburb or to the Tel Aviv area), the national religious, and the ultraorthodox. The unrepresentativeness of the capital's population with respect to the national profile, reminiscent of Washington, DC, also may be grounds for analyzing even Jewish Jerusalem comparatively.

By contrast, Benvenisti's work, which unlike Sharkansky's covers ground well after the Oslo peace process, reads like an historical and intellectual essay in the postmodern style and focuses to a much greater degree on the national and religious conflict between Palestinians and Israelis. His style is certainly a matter of choice rather than necessity, since Benvenisti's earlier works relate to comparative studies on deeply divided cities. The book begins, like many of this genre, on the theme that history is constructed rather than discovered as it leads the reader through a tour of the Israeli-run City of David Museum, housed in a former citadel and mosque built by the Ottomans, who ruled Jerusalem until 1918. Benvenisti's point is well taken. He could have improved upon it by adding that the citadel appears on state television to evoke Jewish Jerusalem, or by noting that Israel's unofficial national dish, felafel, was a traditional staple of its Arab protagonists long before the Ashkenazi Zionists from Europe ever encountered it. His frequent references to scripture regarding Jewish-ruled Jerusalem give the impression that the author is not entirely convinced that all history is equally constructed by the various protagonists.

Benvenisti also is postmodern in his disregard at times for academic convention, which in the quest for objectivity calls for minimizing the use of inflammatory adjectives. Thus, Jews aspiring to pray on the Temple Mount are often described as fanatical extremists, as if the word *extremist*, problematic in its own right, is insufficient. One can claim, of course, that this hyperbole reflects the symbolic nature pervading politics in Jerusalem and is thus an "objective" representation of reality. He lauds Moshe Dayan's decision to prevent Jews praying on the Temple Mount "in order to prevent a religious war whose consequences were beyond conjecture," failing to point out that where the status quo was violated and Jews were accorded equal access and rights to prayer in the Cave of the Patriarchs (al-Haram al-Ibrahimiyya), tension ensued but not nearly to the point of war. After all, the Palestinian national movement, the major protagonist, has the most to lose from transforming the Temple Mount issue into the focal point of the struggle, which could only serve to strengthen the Palestinian religious opposition. Neither do religions, certainly not an unorganized one such as Sunni Islam, possess armies to wage war, as critics have noted regarding Samuel Huntington's thesis on the clash of civilizations. These lapses are unfortunate, as the author's treatment of the conflict is generally even and fair and often brilliantly insightful.

Especially useful is Benvenisti's summary of the four models of governance that have been used or proposed for Jerusalem. Unfortunately, the author does not discuss in sufficient detail how they relate to the larger issues of sovereignty and power sharing, nor is there any reference either to Jerusalem's history or to the comparative literature to suggest which of the generic models may be more useful. His pronouncement that "the real test of a formula for a solution is its ability to accommodate to changing circumstances, but even more essential is the willingness to accommodate to changing circumstances" (p. 133) may be true but is insufficient. Institutionalized forms of governance are equally if not more important than either attitudes or informal government.

A final and related point: Metaphysic and religious feelings are an important dimension of politics surrounding Jerusalem. Yet, in the final analysis, it is not the dead in the city's graveyards or the sects that bury them which dictate politics, as the author suggests in the last chapter, but rather the struggle over one of the largest and most strategic cities in the area between a nation-state that quite naturally wants a united city as its capital and a national movement that may be willing to share it in its quest for statehood. More political science treatment at the expense of an infinitely more elegant literary analysis might be more useful in fleshing out the nature of this rather earthly conflict and solutions to it. A worthwhile project would now be to bring out an edited volume with articles by scholars of other deeply divided cities. Sharkansky and Benvenisti both merit editing it, or at the very least writing its concluding chapter.

Taking on Goliath: The Emergence of a New Left Party and the Struggle for Democracy in Mexico. By Kathleen Bruhn. University Park: Pennsylvania State University Press, 1997. 365p. $55.00 cloth, $22.50 paper.

Charles L. Davis, *University of Kentucky*

Following the 1988 presidential election, many commentators and even officials of the ruling Partido Revolucionario Institucional (PRI) were heralding the end of one-party rule in Mexico. A coalition comprised of a dissident faction of the PRI and several minor opposition parties mounted the strongest electoral challenge yet to continuing PRI hegemony. Following the election, the coalition known as the Frente Democrático Nacional (FDN) sought to consolidate as a new left-of-center party under the leadership of its presidential candidate, Cuauhtémoc Cárdenas, a former leader of the dissident fraction of the PRI and son of one of Mexico's most reverred presidents (Lázaro Cárdenas, 1934–40). This new party, known as the Partido de la Revolución Democrática (PRD), suffered severe electoral setbacks during the presidency of Carlos Salinas (1988–94). In *Taking on Goliath*, Kathleen Bruhn seeks to explain why the PRD was far more successful at emergence than at consolidation and to discuss the implications for democratization in Mexico.

Social and political conditions were favorable for the emergence of a new party in 1988. Voters had become detached from their loyalties to the ruling party, creating a potential reservoir of support for a new party. The incumbent administration of Miguel de la Madrid had become unpopular because of the continuing economic downturn, the imposition of austerity measures, and growing dissatisfaction with political corruption. Moreover, the ruling elite had become fractured, and a dissident faction of the PRI broke from the party to form the new coalition. The situation was ripe for a broadly based coalition of parties under the charismatic leadership of Cárdenas to mobilize a strong protest vote. Yet, formation of a new party does not ensure its consolidation. Bruhn argues that the conditions facilitating emergence can hinder its consolidation.

Bruhn identifies several tasks that must be performed if a new party is to become consolidated. Two organizational tasks are to institutionalize the party and to form effective relationships with key actors in the political system. Also, the party must bring about a reattachment of party loyalties so as to alter established patterns of electoral contestation. While emergence depends on partisan detachment, consolidation of a new party requires that voters move from detachment to reattachment. In the terminology of the voting behavior literature, dealignment must be replaced by realignment. Basically, her argument is that the PRD has largely failed at both institutional consolidation and reattachment of voters. As a consequence, the PRI could "reassert control over democratization" (p. 307) following the 1988 election and ensure top-down control over regime liberalization.

The task of institutionalizing an activist network within the structures of the PRD was complicated by the fusion nature of the party's emergence. The heterogeneity of groups and movements backing the PRD, combined with the personalist structure of activist networks, made it difficult to establish stable norms and procedures to govern the party. Attempts to institutionalize internal democracy further exacerbated factional conflicts and divisions and led to intense conflicts over the rules of the game. The charismatic leadership of Cárdenas that had contributed to electoral success in 1988 tended to undermine institutional development. Moreover, the PRD confronted an institutional context in which the ruling party controlled far greater resources that could be used for cooptation.

Bruhn shows that the PRD also failed to form effective linkages with critical external actors. These included popular movements that had supported Cárdenas in 1988, PRI officials and government agencies whose cooperation was essential for incumbent PRD elected officials, and other parties whose support might have forestalled the party's marginalization. The PRD simply lacked the cooptive resources of the PRI to maintain influence with popular organizations. Moreover, the very heterogeneity of its bases of support decreased the capacity of the party to keep diverse groups satisfied. Bruhn shows how PRD elected officials were hampered in providing effective leadership by their marginal position in Mexico's highly centralized political system. In like manner, the confrontational style of the party and ideological incompatibility with both the PRI and the conservative Partido Acción Nacional (PAN) prevented the formation of strategic alliances and effective negotiations with the other major parties.

The declining fortunes of the PRD were revealed by its dismal performance in both the 1991 mid-term and the 1994 presidential election (chapter 7). The author attributes the declining electoral fortune of the PRD to a failure to bring about a reattachment of voters that would have strengthened its electoral base. Though recognizing that the PRI also failed to prevent growing dealignment of voters, Bruhn argues that it was in a good position to capture the support of the detached bloc because of its control over institutional and legal resources and improvement in macroeconomic performance during the Salinas years. Moreover, Bruhn claims the PRD was hurt by failing to achieve institutional consolidation and by clinging to "a social movement logic rather than an electoral logic" (p. 253). The social movement logic was shown in the confrontational strategy adopted toward the ruling party and government, support for radical groups like the Zapatistas, and reliance on electoral rallies to mobilize voters. These factors presumably weakened the capacity of the PRD to capture the decisive bloc of independent voters.

This book is must reading for scholars with an interest in contemporary Mexican politics and society. It is the definitive study of the emergence and demise of the PRD in a few brief years between the late 1980s and early 1990s. The field research that went into this study is exhaustive, even if the case studies of party organization of the PRD are limited to the states of Mexico and Michoacán. Extensive interviews with participants and the use of archival materials along with survey data allow Bruhn to provide an in-depth study not only of a newly emergent party but also of a hegemonic party system in the midst of profound change.

Still, there are some questions to be raised about Bruhn's analysis of the PRD and the Mexican political context. It is not at all clear that the confrontational strategy of the PRD was necessarily irrational, even if considered from "an electoral logic." The ruling party was clearly in a position to stack the deck against the PRD after the 1988 election. As Bruhn shows, this is exactly what did occur. Perhaps this strategy alienated the large detached bloc, but even so, it is not clear whether the PRI still would not have made significant inroads with this bloc due to economic recovery and the party's superior institutional resources. Moreover, a more cautious, moderate strategy may well have alienated the ideologically motivated activist base of the party and of popular movements. My own research in progress suggests that the attached Perredistas tend to be more alienated and ideologically motivated (and more politically involved) than are the detached. In short, the pursuit of a more moderate electoral strategy could have cost the PRD support from its activist base and long-term loyalists. Also, it is not at all clear that opposition parties in a hegemonic party system can negotiate a more level playing field except through confrontation. This strategy is precisely what was pursued by earlier leftist opposition movements in Mexico, as the author shows.

Bruhn gives a great deal of attention to party institutionalization and institutional advantage as critical variables shaping the electoral fortunes of Mexican parties. It is difficult to take exception with this point. It is becoming increasingly clear, however, that macroeconomic conditions can undercut the institutional advantages of the PRI. Moreover, integration of the Mexican economy into the global economy has undermined the capacity of the state to control economic outcomes. This capacity has also been eroded by neoliberal reforms that have decreased the economic role of the state. As a consequence, the electoral prospects of the PRI are increasingly dependent on macroeconomic conditions over which the incumbent government has declining control. As witnessed in the 1988 election and elections since the peso devaluation crisis of 1995, economic deterioration strengthens the hand of opposition parties. Hence, it would seem premature to pronounce the death of the PRD simply on the basis of its dismal performance in the 1991 and 1994 elections. Changing macroeconomic conditions could yet give David an opportunity to slay Goliath.

Bruhn carefully avoids sweeping generalizations from her case study of the fate of the PRD in the context of Mexico's changing hegemonic party system. Yet, the theoretical contribution of the study would have been enhanced by more comparative analysis. Is consolidation of a new party more likely if the party is nonleftist and located in a more competitive electoral system, as Bruhn suggests? Some empirical analysis of other cases, either qualitative or quantitative, would have been useful for understanding how the Mexican case fits into the larger picture. Clearly, some leftist parties in Latin America (Causa R in Venezuela or the Workers' Party in Brazil) have been more successful than the PRD in achieving electoral success and institutional consolidation. Can Bruhn's theoretical framework account for these cases? Some interesting theoretical discussion and comparative analysis are provided on modes of democratic transitions in hegemonic party systems and the difficulty of pacted transitions in such systems (chapter 7). More analysis of this sort, particularly on the fate of leftist parties that challenge the neoliberal direction of economic policy in Latin America or more generally in the Third World, would increase the appeal of this book to scholars whose interests extend beyond Mexico.

Social Movements, Political Violence, and the State: A Comparative Analysis of Italy and Germany. By Donatella Della Porta. Cambridge: Cambridge University Press. 1995. 270p. $59.95.

Taking Lives. By Irving Louis Horowitz. New Brunswick, NJ: Transaction Publishers. 1997. 324p. $44.95 cloth, $24.95 paper.

Terror, Force, and States: The Path from Modernity. By Rosemary H.T. O'Kane. Brookfield, MA: Edward Elgar. 1996. 214p. $79.95.

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Each of the books reviewed attempts to explain why, amid numerous options, violence is selected by different political actors. Della Porta principally examines dissidents, while O'Kane and Horowitz deal with states. Although the actors considered vary, all the books basically address the same fundamental question, employ similar methods, and reach similar conclusions. In fact, the three fit so well together that, read cumulatively, the understanding of political violence is advanced significantly.

What makes these varied examinations important is that they each place the issue of violence at the center of their discussions. Violence is not marginalized, as it has been within the social sciences generally and within political science in particular, seen as some deviant act undertaken by crazy, demented, or "evil" people. Rather, violence is placed into the realm of possible alternatives consistently available to diverse political actors within nation-states, and it is examined (rightfully, I believe) as something that exists at the center of political life itself. While unified on this basic theme, however, the differences within the books both clarify our understanding of violence and also highlight the difficulty in studying it. This will become clearer below.

Della Porta investigates the most narrowly focused research question of the three books considered: She seeks to understand why specific groups were likely to use political violence within Italy and Germany from 1960–90. Fundamentally, she suggests that violence is selected and used by dissidents with increased frequency because of a particular sequence of actions, which result in the isolation and desperation of these political actors. The theory developed intricately weaves together variables from micro, meso, and macro levels.

As outlined, those challenging existing political-economic relations begin their activities within a larger movement "family" (a group of social movement organizations that emerge at one time). These families adopt a relatively wide repertoire of strategies—both violent and nonviolent. Over time, some strategies fall to the wayside (because of concessions given to dissidents, repression, and/or the exhaustion of dissident members themselves), while others become more prominent (because of effectiveness or other dynamics, discussed below).

Violence emerges as a dominant political strategy within the repertoire employed by dissidents because specific organizations become marginalized (by their objectives and actions) as well as radicalized (again by their objectives but also by interactions with repressive agents). Simply, isolated because of the decreased activity of the movement family (with nothing left to do) and angered by being treated in a generally hostile manner (receiving even more repressive behavior than before), some dissidents employ violent political strategies as a means of furthering their objectives (some combination of political influence, survival, and personal meaning). The strategy itself becomes further reinforced by various internal dynamics to dissident organizations (e.g., the ability to draw upon those unsatisfied with other groups) as well as personal dynamics among different members themselves. Initial choices, principally guided by macro- and mesolevel factors, thus yield to certain pathologies, which are principally guided by meso- and microlevel factors. These in turn become extremely hard to stop.

O'Kane also provides a relatively focused investigation of why states employ violence (terror) in the governance of their populace. Similar to Della Porta, O'Kane argues that violence is functionally related to a specific sequence of actions which result in the isolation and desperation of these political actors. From her perspective, "dislocation" (a breakdown of "traditional networks," identification of scapegoats, and an economic crisis) provides the initial impetus for violent political strategies. Within this context, the desire to (re)establish order and to pursue specific political-economic objectives becomes increasingly important. In fact, it comes to overwhelm all other objectives.

Generally lacking support within the populace for proposed objectives, states employ violence as a means toward bringing them about. This practice is facilitated by propaganda as well as an unresponsive ("infallible") leader who pushes the agenda despite negative feedback from the society/business community about the actual effectiveness of the political-economic policies undertaken. The violent strategy begins to develop its own self-reinforcing dynamic; specifically, violence (terror) is applied because it is believed that it will achieve certain objectives and because no other means exist for information about its ineffectiveness to influence subsequent decisions. Again, initial choices yield to certain pathologies that become extremely difficult to stop once underway; indeed, according to O'Kane, collapse of the regime is the most likely (and perhaps only) way out.

Horowitz takes a somewhat different approach. He principally addresses the specific question of why states use violence (genocide). He also asks broader questions of why it is important to study violence, how one should study it, and what interferes with systematic investigation. For this review, I will basically concern myself with the first point (although each of the others is worthy of attention in its own right).

Like Della Porta and O'Kane, Horowitz sees the political strategy of violence as related to isolation, but he moves away from suggesting that the isolation (i.e., dissident organizations from social movement families in the case of Della Porta or states from citizens in the case of O'Kane) is important to an argument that the isolation of all actors within society contributes to violence being selected by states. Horowitz views two factors as important: (1) a diminished respect for individuals with an accompanying neglect of certain obligations to other citizens and (2) a diminished sense of "biblical" justice: punishment for individual transgression of particular laws. Both contribute to a society that is generally uncaring about individual citizens and is more concerned with the "collective will" or "collective conscience." Indeed, within Horowitz's argument it is the subservience of individual citizens as well as the state to the altar of collectivism that proves to be the most important determinant of state violence (fueled by the increased capacity of states to enforce their will through technological advances).

All three studies are important for studying violent behavior undertaken by different political actors, for each highlights the significance of developing generalizable theories about the subject. Della Porta and O'Kane take a more traditional approach and apply their theories to two cases extensively (Italy and Germany as well as the Soviet Union and Nazi Germany, respectively), later extending them to other cases (Della Porta to the civil rights and student movements, although the black liberation/nationalist movement would have suited her argument better, and O'Kane to the Cambodian case). By contrast, Horowitz seems to develop certain theoretical arguments without extensively applying them to specific cases. More abstract in intent and approach, his effort tackles some of the categorical, philosophical, practical, and personal difficulties confronted when studying political violence.

In addition, all three studies are important because they apply distinct bodies of research (Della Porta draws upon social movement literature, O'Kane draws upon work on power and "closed" political systems, and Horowitz employs the work on terror and genocide). Each also incorporates different levels of concern for use of empirical data (Della Porta spends a great deal of time on operationalization/measurement issues, O'Kane employs none, and Horowitz uses none himself but clearly highlights the importance of these efforts). These points are relevant, since the different areas of research guide the authors in directions unique to that particular tradition, and the manner in which the empirical issue is handled directly facilitates/impedes other scholars who may wish to investigate rigorously the theoretical arguments presented.

Such diversity in approach leads to the obvious suggestion that more integrative efforts should be pursued in the future. For example, Della Porta clearly and appropriately uses the work on social movements (based in sociology) but is basically silent on the "state's side" of the interaction (based in political science). This is unfortunate, for she suggests in several sections of the book that protest "policing" is extremely important in bringing about dissident political violence. I would even go so far as to say that this variable is the most important factor identified. But, this having been said, the literature on state repression, human rights violations, and state terror is not brought into the discussion.

By contrast, O'Kane and Horowitz, while clearly and appropriately using work on power as well as state terror/violence, are basically silent on the "dissident/citizen side" of the interaction. The importance of this omission is significant because one element within all three books is that violence is a by-product of a specific interaction between states and their citizens, principally around the subject of control/influence over domestic politics. In fact, this appears to be the underlying element that starts both dissidents and states on the path of violence in the first place.

On this point, Della Porta comes closest to suggesting that violence is an outcome of competition between states and dissidents/citizens for the control of the polity. Horowitz also comes close to this point when he comments that genocide is war. He neglects, however, to identify/discuss the nonstate combatants. Despite these efforts, neither author takes the step to suggest that violence is intricately connected with a quest for intrastate domination. This point, while perhaps sounding extreme, seems to lie at the core of an explanation for state violence.

This desire to stay away from what I will call a Marxian or Libertarian conception of state-societal relations (as but two well-understood examples of perspectives that might emphasize such a conflictual interaction) follows directly from all three authors' belief that greater amounts of democracy will reduce violent political strategies. Admittedly, there is some diversity here, as different aspects of democracy are highlighted: Della Porta seems to stress accommodation or less repressive responses to dissidents; O'Kane seems to stress greater feedback between states and societies; and Horowitz seems to stress greater respect for individual rights. Regardless of the particular operationalization, however, all aspects are found within the basic tenets of the democracy literature, and one is left with the resounding conclusion that democratization reduces violence (directly in line with the work of R.J. Rummel).

The democracy-as-solution position is important because it tends to lead the authors away from the starting point: Violence lies within the parameters of normal political existence. In a sense, each author explains political violence by revealing how dissidents and states come to move away from nonviolence, without returning to the point of how easy it is to begin on this path. Moreover, the hopefulness of democracy-as-solution tends to ignore the problem of how political systems move toward or truly reach democracy after political violence has taken place. Would violence not decrease citizens' willingness to participate in political or social life (as illustrated in Corradi et al., *Fear at the Edge: State Terror and Resistance in Latin America*)? Would violence not change individual perceptions of efficacy and trust in government, damaging any prospects for healthy democratic practices? Do not repressive legacies exist long after the last person has been killed, influencing the capacity of society/government to function? Simply, the larger aftereffects of the pathologies and behaviors identified within the three books need to be addressed as well as their enduring influence in order to provide better potential resolutions to the problem.

I do not fault the authors of the three books reviewed on this last point, for it was not what they intended to do. Indeed, each provides useful insights into violence. One cannot help but conclude, however, that if we are to further our understanding of political violence and perhaps even attempt to decrease it (if at all possible), then taking on the questions left unanswered by these books should be pursued with the utmost seriousness and diligence.

The Transformation of Property Rights in the Gold Coast: An Empirical Analysis Applying Rational Choice Theory.

By Kathryn Firmin-Sellers. New York: Cambridge University Press, 1996. 200p. $54.95.

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In this succinct, clear, and well-written book, Kathryn Firmin-Sellers offers a rational-choice explanation of the transformation of property rights in the Gold Coast. Her main argument is that property rights under British colonial rule remained insecure and fluid and thus caused the failure of capitalist development. Secure property rights, she contends, rest on a combination of both institutional coercive capacity and institutional political incentives. Rulers must have the power to enforce a particular set of property rights and deny any alternative system, but they must do so without using that power to create an allocation of property rights that benefits them exclusively. Effective enforcement requires a state strong enough to sanction violators but not so omnipotent as to change property rights as it pleases. According to Firmin-Sellers, economic growth and capitalist development are the logical consequence of such a situation.

So the fundamental question becomes: Why did secure property rights fail to materialize in the Gold Coast? Firmin-Sellers contends that because British colonialism sought to minimize the costs of domination and maintain stability, it had to rely on traditional African chiefs to execute its policies. This, in turn, meant an ambiguous relationship between British authorities and African intermediaries. While the former were the "principal," they had to delegate some power to the latter, the "agent." In the process, the principal came to depend on the agent for information with which to make policy. The agent therefore acquired some authority, even if it was limited and circumscribed by the colonial state.

The ultimate consequence of the principal/agent relationship was the incapacity of British colonial administrators to wipe out all autonomous sources of indigenous authority. Indirect rule preserved customary practices, albeit in a new political and material framework. African chiefs were indispensable for minimizing the costs of empire, but they also created their own independent network of personal power and thus emasculated the empire's absolutist pretensions.

While the agents had some real influence, they had to capture the support of the colonial state to enforce whatever social project they may have entertained. Thus, in the view of Firmin-Sellers, the capacity of African chiefs to obtain such support was determinant in their attempts to benefit from the capitalist opportunities the colonial system offered. Chiefs sought to establish secure property rights from which they might derive material gains, but in this endeavor they did not always get the support of colonial authorities. When they did, the population prospered; when they did not, economic stagnation ensued. This at least is the lesson that Firmin-Sellers draws from her two case studies. She analyzes how the respective efforts of indigenous elites in the Ga state and in Akyem Abuakwa to enforce a single property rights system resulted in failure and success.

To make her case, Firmin-Sellers focuses on the micro-level analysis of preferences and beliefs of two prominent citizens: Ga's Dr. C. E. Reindorf and Akyem Abuakwa's Dr. J. B. Danquah. As paradigmatic figures of the elites of both states, they demonstrate clearly in Firmin-Sellers's view that African traditional rulers were determined to pursue "greater wealth and power by redefining custom so as to encourage, not discourage, productive, market oriented investment" (p. 146). This "re-invention of tradition," entailed inevitable distributional conflicts and thus generated indigenous opposition, which the elites had to defeat if they were to succeed. In Ga, the Reindorfs failed to settle the dispute over custom because they lacked the coercive means to enforce a system akin to private property rights; they simply did not "capture" the colonial state for achieving their ends. In contrast, in Akyem Abuakwa, the Danquahs were able to establish their preferred system of property rights because it "safeguarded each citizen's access to state lands and allowed citizens to profit from their investment in the land, even as it created a closed market for that land" (p. 88). Moreover, the Akyem Abuakwa elites could capture the colonial state to enforce the new system because the weaker indigenous agents had consented to abide by it and thus minimized the costs of enforcement.

The general conclusion of Firmin-Sellers is thus that principals must not only have the capacity and will to coerce others into conforming to institutional rules but also must restrain from violating these rules when that may benefit them; only then can society prosper. But why would anyone temper self-interest in the name of the common good? This question is specially pertinent for Firmin-Sellers, since she claims to base her analysis on rational-choice principles whose logic prevents that very outcome. In fact, she rejects this strict logic; she accepts the rather old idea that collective benefits are ultimately generated by distributional struggles and by the relative balance of power between rulers and ruled.

In this perspective, it is not surprising that in a colonial situation like that of the Gold Coast distributional conflicts resulted in few benefits for local inhabitants. Colonialism, as Crawford Young (*The African Colonial State in Comparative Perspective*, 1994) has convincingly reminded us, was not a benign enterprise giving significant choices to African actors. Built on an acute racism that denied Africans their humanity, colonialism was above all a system of massive extraction; it was not bent on the development of the continent. That some Africans benefited from their collaboration with colonial authorities does not imply that they had real meaningful choices in determining their social projects. They were always in a dependent situation vis-à-vis colonial rulers; any coincidence of interests that materialized between the two was a purely accidental opportunism.

Unfortunately, this book tends to minimize the ruthlessness of colonialism and presents clear patterns of subjugation as mere relations between principals and agents. Yet, it does show convincingly that, even under colonialism, Africans were not a homogeneous sociological category; they were deeply divided over distributional issues and sought to maximize the minimal opportunities colonialism offered. Moreover, Firmin-Sellers challenges convincingly the idea that only private property rights engender sustained economic development and that customary law inhibits it. What really matters, she argues forcefully, is the fluidity of property rights; insecure rights lead to stagnation, secure rights to prosperity.

All in all, Firmin-Sellers has written an interesting book that will please those who subscribe to a revisionist version of rational-choice theory. For others, the book will still leave doubts about the applicability of this theory to the study of colonial domination in Africa. All readers, however, will benefit from her thoughtful and well-researched exposition of the evolution of property rights in the Gold Coast.

Voting for Reform: Democracy, Political Liberalization, and Economic Adjustment. Edited by Stephan Haggard and Steven B. Webb. New York: Published for the World Bank by Oxford University Press, 1994. 519p. $19.95.

Global Change, Regional Response: The New International Context of Development. Edited by Barbara Stallings. Cambridge: Cambridge University Press, 1995. 410p. $59.95 cloth, $18.95 paper.

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These two edited collections explore the influence of political context on economic development outside the advanced industrial democracies. *Reform* examines the ways in which domestic political regimes, bureaucratic and political party institutions, and the interests of politicians, technocrats, and key constituent groups shape the implementation of economic reform programs. The premise of *Global Change* is that historical and geopolitical ties between capitalist hegemons and their respective semiperipheries have, in the post–Cold War world, become ever more determinant of economic opportunity for those countries not historically members of the Organization for Economic Cooperation and Development (OECD). *Reform* does a good job of identifying trees—the editors and contributors adduce numerous fascinating hypotheses, and the case studies are intelligently selected goldmines of detailed empirical information about the politics of particular economic reforms—but avoids much comment on the forest, perhaps for its own political reasons, since the World Bank funded and published the study. (On the forest, see Stephan Haggard and Robert R. Kaufman, *The Political Economy of Democratic Transition*, 1995.) *Global Change*, in contrast, makes bold, easily understood, well-argued, and often categorical statements. Stallings, currently research director at the United Nations' Economic Commission for Latin America and the Caribbean (the Santiago, Chile, think tank best known for the theories of its longtime former director Raúl Prebisch), has articulated what might be termed the new neostructuralism, which like its predecessors explains how and why the supposedly neutral global market does not treat all comers (nation-states) equally.

Reform includes three initial theoretical chapters and eight exhaustive case studies of the implementation process of two kinds of economic reforms: inflation stabilization/fiscal austerity and trade liberalization (usually including devaluation and sometimes also part of a larger structural reform package including domestic deregulation and/or privatization). Haggard and Webb put forth a number of serious, policy-relevant hypotheses, looking to the case studies for illustrations. New democracies that inherit macroeconomic chaos, as in Poland, do best with rapid, radical economic reforms which can be implemented during their political honeymoons, rather than gradual reforms which will be nibbled to death by opposed interests (pp. 7–8, 21–3). New democracies that follow economically reforming authoritarians, as in Chile, can consolidate stabilization through political pacts between departing and incoming governing elites (p. 7). Polarized party systems, or ones with multiple veto points (see also the chapter by Alesina, p. 53), are detrimental to the consensus-building among relevant political actors required for successful economic reforms (p. 9). Strong, even hegemonic, ruling parties, as in Mexico and Senegal in the 1980s, can aggregate diverse interests, many of which otherwise would oppose reform (p. 11). Incumbents rarely initiate transitionally painful structural adjustment programs prior to elections (p. 12), unless, as in Mexico, the ruling party believes its electoral success is assured (p. 13). Both inherited administrative "capacity" and insulated decision making (including situations in which the chief executive surrounds himself with a newly created agency or a kitchen cabinet) increase the likely success of economic reform (p. 12). In the long run, however, reforms must become popular (p. 12), at least with those interests that can wield political resources. Labor almost always comes out a loser, at least during the transition period. Institutions that keep labor peace, including corporatist arrangements under democratic rules (and, unfortunately, repression under authoritarianism), thus facilitate economic reform (p. 17).

The chapters by Alberto Alesina and Dani Rodrik, although drawing upon formal theory in contrast to empirical evidence, also yield numerous hypotheses about the political economy of policy implementation. Alesina asserts that all groups gain from inflation stabilization and rightly rejects the hypothesis that policymakers and interest groups in developing countries do not stabilize because they are naive or stupid. He thus deduces that delays in stabilization occur because (1) the distribution of transitional losses during policy reform is uneven across different economic interests in society, (2) more than one possible distribution of transition costs could achieve stabilization (therefore, each group will try to achieve the cost distribution most beneficial to itself), (3) a given group does not know other groups' costs of continued inflation (and thus cannot assess when each will capitulate and unilaterally bear the lion's share of the costs of stabilization), and (4) political and institutional arrangements may make solving this quintessential collective-action problem very tough. Concretely, Alesina suggests that weak dictators fearing imminent overthrow are likely to produce some of the worst macroeconomic policy outcomes, because their extremely short time horizon makes both personal corruption and spendthrift political patronage highly attractive (p. 46). Yet, pacted political transitions from authoritarian rule to democracy (in which informal bargains between departing and incoming political elites minimize both societal polarization and effective interest group pressures for new government spending) may yield some of the best results.

Rodrik gives us reason to wonder that trade liberalization ever occurs. Unlike inflation stabilization, in which everyone is better off once the transition costs have been paid by whomever is politically weakest, under trade reform those groups that previously were receiving oligopoly rents from protectionism now will be made worse off. (I note in passing that bizarre institutional contexts do exist in which certain groups stand to lose absolutely from stabilization. See my "Inflation and Insouciance: The Peculiar Brazilian Game," *Latin American Research Review*, 31 (3), 1996, pp. 7–46.) Losers, naturally, will scream if they believe that they can be heard. Rodrik constructs a political cost-benefit ratio to show that politicians get precious little net benefit for themselves from pursuing commercial opening, although devaluation could be politically useful in the frequently occuring circumstance that labor—especially nonunionized and informal sector labor typically concentrated in the nontradables sector (pp. 69–72)—is the biggest net loser. He concludes, in common with Haggard and Webb, that trade reform is most likely to occur when it is packaged with other, more popular programs.

The eight case studies also are individually very good. What is missing is a deeper dialogue among the theoretical and country chapters. For example, six of the countries (Spain, Turkey, Poland, Chile, Mexico, and Thailand) appear to illustrate successful implementation of economic reform, while two (Senegal and Nigeria) are failures. None of the theoretical chapters has a systematic explanation for this outcome, except to note that it is surely not culture and may have to do with the lack of an insulated technocracy ideologically commited to reforms. Samba Ka and Nicolas Van de Walle describe political and technocratic elites in Senegal who were agnostic about the economic virtues of the reforms (pp. 292 and passim) and who thus evaluated reforms on the basis of their (dubious) political utility. Meanwhile, the commited neoliberal technocrats in Nigeria's central bank had no power (p. 495, in the chapter by Jeffrey Herbst and Adebayo Olukoshi). *Global Change*, incidentally, does offer an explanation for the poor showing of sub-Saharan Africa, of which more below.

The case studies, for which I could not locate an explicit selection rationale, offer an interesting implicit commentary upon the theoretical hypotheses in the early chapters. Collectively, the eight country chapters suggest that the dimensions highlighted in most of the explanatory hypotheses, while relevant, are neither sufficient nor even necessary to explaining the success or failure of economic policy reform. There is nothing really wrong with this conclusion; in a world where sixteen variables always change simultaneously, perhaps the best to hope for is to point to the six really important ones. But the synthesis still could have been neater. For example, strong ruling parties with secure mandates (whether due to elections or clientelism), which supposedly make economic reform more likely, were present in three successful cases (Spain, though only in the second democratic administration, Poland, and Mexico) and also in one failed case: Senegal. Strong parties did not figure in three other successful cases—Thailand, newly democratic Chile in the early 1990s, and Turkey. In fact, Richard F. Doner and Anek Laothamatas take the absence of a strong political party at the top and the presence of considerable political instability as a central puzzle of their chapter on Thailand (p. 411). They conclude that political uncertainty actually aided economic reform, because it allowed the democratically elected president, General Prem, to act as an honest broker among factions, including both the military (which disliked devaluation, but was more afraid of overt political turmoil, and thus supported Prem) and the civilian opposition (which felt constrained to support the president, since the alternative could be an explicit dictatorship) (pp. 412 and passim).

Another apparent anomaly is that the chapters by Alesina and Rodrik seem to suggest that commercial liberalization is intrinsically tougher than bringing down inflation, yet the case studies show that Turkey, Mexico, and even Nigeria did better on the former than the latter. We might also consider the question of economic shock treatment, or rapid, comprehensive reform—which Haggard and Webb appear to endorse, although perhaps only in cases of profound economic crisis?—versus gradualism. Poland, described here by Simon Johnson and Marzena Kowalska, is the archetypical case of successful shock treatment reform. To a certain extent Chile, which had rapid, sweeping deregulation in the early to mid-1970s, also illustrates the possibilities of shock treatment. It may be noted, however, that all the other more or less successful cases appear to show some virtues for gradualism. Nor is it clear that a faster pace of change would have altered the outcomes in the two nonreform cases. Moreover, Chile's economic reforms were sustained only by a murderous dictatorship and were followed by two massive macroeconomic meltdowns in 1975 and again in the early 1980s, not to mention fifteen years of high formal sector unemployment (worse in the informal sector) and wage levels that in 1989 still had not recovered to their levels before the 1973 coup (p. 247). Genaro Arriagada Herrera and Carol Graham, incidentally, indicate that it was not only the pacted political transition per se that sustained Chile's neoliberal reforms into the 1990s but a combination of the eventual return of strong macroeconomic growth (which predated redemocratization) and the continuing strong ideological commitment of the outgoing authoritarians, who made their cooperation contingent upon economic policy continuity. Somewhat surprisingly, in fact, of the two books under review, it is *Global Change*, whose overall analysis is global and structural, that gives the greater sustained weight to ideas as an explanatory force, particularly but not exclusively in the chapter on the spread of neoliberalism written by Thomas J. Biersteker.

Finally, Haggard and Webb assert that, in the book's cases, international pressures and opportunities mattered only at the margin (p. 25). This conclusion only is valid if one defines "international" factors as simply the promise or absence of foreign aid, ignoring the need to attract private foreign investment (granted, less important in cross-border capital flows before 1990, when the bulk of these reform episodes occured) or achieve other international goals. For example, the authors of the chapters on both Spain (Nancy Bermeo with José García-Durán) and Turkey (Ziya Onis and Webb) point to the lure of qualifying for membership in the European Union as a critical motivator of policy reform.

All these comments are to be understood as quibbles with a book that I found very valuable. I do have one clear criticism: I object to the book's title and the implied benign and democratic world of economic reform that it invokes. None of the case studies, much less the theoretical chapters, describes the ordinary voter or citizen as anything but marginal to the grand societal, and sometimes international, tussles over economic policy reforms among competing elites and dominant interests. None of us can change this fact, but it is unfair not to acknowledge it honestly and regularly—as the editors do remember to do from time to time (see, for example, p. 24).

The book organized by Barbara Stallings, in contrast to the highly specific country stories and provocative but disconnected hypotheses of the Haggard and Webb volume, offers an ambitious and organized prism through which to interpret the disparate experiences of particular countries. It is a debate-defining contribution: clear, coherent in themes across different authors, stocked with relevant and hard-to-come-by statistics describing recent shifts in the global economy, and often very persuasive.

Part I looks at five global changes: the end of the Cold War (in the chapter by Fred Halliday); competition and contrasts among three forms of capitalism, the U.S., European, and Japanese varieties (Stallings and Wolfgang Streeck); global private production and distribution networks differently managed by transnational corporations based in the three capitalist centers (Gary Gereffi); the rapid expansion of and qualitative shifts in cross-border financial transactions (Stephany Griffith-Jones and Stallings); and the spread of neoliberalism to the developing world (Biersteker). Part II describes four sets of regional responses to these changes. Yun-han Chu analyses the book's most obvious cases of economic success: the East Asian newly industrialized countries (NICs), who have greatly benefited from Japan's tutelage. Linda Y.C. Lim shows how the countries of Southeast Asia, despite their much greater economic openness as compared to Japan and three of the four early tigers, also profited from Japanese investment, hard and soft technologies, and marketing expertise and access. Augusto Varas, describing the geographic area most closely linked to the United States, details Latin America's wholehearted, and often not so hardheaded, embrace of deregulation and free markets (Cuba, and to some extent Brazil, excepted), even in the absence of the hoped-for quantities of investment from and market access to the United States. Michael Chege identifies the development tragedy of sub-Saharan Africa as a combination, first, of being forgotten once the Cold War ended and, second, of the continuing lack of interest of private businesses hailing from its regional hegemon, Western Europe (mainly Britain, Germany, and France), in transferring capital, production, and hard and soft organizational technologies to African environments.

While intermittently chiding the United States for its abysmal domestic savings rate and high external deficit, Stallings notes that rich countries have two main ways of influencing their peripheries (a label she eschews, but which still seems to fit): by investment flows, as the Japanese do, or by market access to high-income consumers, in which type of economic power the United States remains king (p. 351). Each regional economic hegemon transfers types of capitalist organization—and especially its unique forms of government-private sector interaction—to the countries in its sphere of influence. The least fortunate countries are those, such as much of Africa and the Middle East, along with parts of South Asia, that have no regional leader to transfer resources and offer concrete integration into global production chains.

Stallings readily acknowledges her intellectual debt to Kenichi Ohmae (chap. 11, esp. pp. 352–4) and other theorists of contemporary capitalism such as Lester Thurow (pp. 71–2). She wonders whether their competing capitalisms model or the global convergence thesis of such scholars as Stephan Haggard and Albert Fishlow (p. 70) better describes the future for the world's rich countries, but she unequivocally states that "the Japanese-dominated Asian region has provided a more supportive environment for development than has the U.S.-dominated Western Hemisphere" (p. 350), and she suggests that the Japanese-led region will continue to be the most dynamic for the foreseeable future. At first this claim seems odd, in the context of Japan's deep troubles in the mid-1990s with recession, property, stock market, and banking crises, prominent failures of sectoral industrial policy, and impending dramatic financial liberalization. Yet, the quantitative evidence the volume provides for the benefits to less-developed countries of having thus far been in the Japanese region are quite persuasive, although anomalies such as the Philippines need to be explained. (This is pretty easy, given the long and deep U.S. presence there.) And Stallings, unlike cruder theorists of Japanese exceptionalism, is not celebrating a particular institutional configuration, but rather an activist mindset on the part of the political and policy leadership (or less benignly, a neomercantilist state). Her general pessimism about the future opportunities for those countries not fortunate enough to claim membership in the (expanding) Pacific Rim is only tempered by the observation that, at least in small ways, the Japanese view of capitalism is beginning to make inroads into the heretofore resolutely neoliberal, U.S.-dominated ideologies of the key international financial institutions (IFIs), the International Monetary Fund and the World Bank.

Global Change, that is, provides at least one possible key to describing the invisible forest behind the studies in *Reform*. The African countries remained clientelistic and chaotic less because of any domestic factors than due to the lack of a strong presence of a foreign hegemon, whose political-economic reach would have included both government-to-government aid and the intrusive but vastly educational presence of the hegemon's transnational corporations. Thailand did better at fiscal reform than trade liberalization (while the opposite happened in Turkey, and has often been true in Latin America) not only because of the domestic interest group and institutional configurations but also because the Japanese model of capitalism is more tolerant of export promotion coupled with de facto import protection than the U.S./IFI model or even the Western European variant. At the same time, most of the contributors to *Global Change*, and certainly its editor, are cautiously critical of the "Washington consensus" (pp. 178, 362–3) and its wholehearted endorsement of free markets, both as economic theory and policy prescription. Taken together, *Global Change* offers an odd but often compelling new version of dependency theory meets the MIT Business School.

Traditional Politics and Regime Change in Brazil. By Frances Hagopian. Cambridge: Cambridge University Press, 1996. 317p. $64.95.

David S. Brown, *Rice University*

When the state was "brought back" in the early 1980s, political scientists began to focus more intently on the constraints political institutions place on political actors. Since then, studies that emphasize the rules of the game in attempts to explain a wide range of social, political, and economic phenomena have proliferated, often at the expense of work that focuses on the actors themselves. Arguments based solely on the rational action of individuals as they respond to changing economic conditions and political institutions are surely too deterministic, ignoring the possibility that political actors can take purposeful action to change the incentives they face. Frances Hagopian's *Traditional Politics and Regime Change in Brazil* tries to swing the pendulum the other way in an attempt to bring politics back in. Hagopian provides an impressive treatment of traditional politics in Brazil, tracing its survival in the face of regime change and a rapidly changing economy. Although the book clearly succeeds in documenting traditional elite survival in Brazil, at a general theoretical level it is somewhat less convincing. Nevertheless, it tackles important issues that Brazilianists and general comparativists will find extremely interesting.

To demonstrate how industrialization and institutional change failed to crack the clientelist nature of state-society interest mediation in Brazil, the book traces the evolution of traditional politics in what has been an important, politically active state in Brazilian politics: Minas Gerais. Hagopian challenges modernizationist and institutionalist arguments based on their failure to predict traditional elite survival during the authoritarian years 1964–85. Massive industrial growth coupled with more than 20 years of authoritarian rule led scholars to predict the eventual demise of traditional politics. Despite these expectations, the same traditional politicians employing the same bag of tricks figured prominently throughout the authoritarian period, inserting themselves into the democratization process. How, under significant economic and institutional change, did the traditional elite survive?

To account for their survival, Hagopian identifies several causes: the nature of state-led capitalism; the military's half-hearted attempt at fiscal centralization; the self-imposed political limits under which the military operated; and the restricted nature of political competition during the authoritarian period. After the 1964 coup, the military increased the state's already large presence in the economy. As state involvement grew, politicians in Minas Gerais gained control over an increasing amount of state resources, which they used to maintain political support. Although technocrats took control in the more technically oriented ministries, traditional elites were left in charge of programs responsible for distributing pork and patronage: education, health, social security, and housing. Their strategic placement within the state bureaucracy allowed the elites to maintain the clientelist ties crucial to their political survival. The military's push toward fiscal centralization also failed to curb their influence. Rather than reducing the traditional elites' power, the military's plan to centralize tax collection and government spending had the effect of depriving small and medium-sized municipalities of revenue, forcing local politicians to rely on elites at the state and federal level. The military's desire to govern constitutionally explains a significant portion of the traditional elites' success as well. Its self-declared adherence to constitutional law compelled the military to hold regular elections. Although it enjoyed support initially—benefiting from high rates of economic growth between 1968 and 1974—the government-sponsored party lost significant electoral support once economic growth began to slow, forcing the military to ally with traditional elites. Though elections kept politics open to some extent, the exclusionary nature of electoral procedures during the authoritarian period allowed the traditional elites to maintain a stranglehold on elite recruitment. To illustrate the traditional elites' ability to thrive under authoritarianism, Hagopian shows that a higher percentage of political offices in Minas Gerais went to members of the traditional elites after the 1964 coup. Authoritarianism not only preserved the influence of traditional politicians but also amplified it. Their new-found power, Hagopian argues, positioned them to play an important role in the initial phases of democratization, allowing them to construct institutions that guaranteed their continuing dominance. This, explains Hagopian, accounts for the slow progress toward genuine democracy in Brazil: a democracy that forces politicians to address the real problems that continue to plague Brazilian society today.

Hagopian is at her best when describing the evolution of traditional politics. The book, however, does not provide a convincing argument for rejecting institutionalist explanations. Although Hagopian is on somewhat shaky ground when generalizing from a single case, her findings should not be rejected simply because there are no degrees of freedom. Nevertheless, missing from her treatment of the traditional elites' role in politics is an attempt to place the findings in any comparative context, temporally or spatially. For example, we are told that "of all eighty members of the political elite who occupied the state and federal posts. . .forty-four (55 percent) 'survived' politically after the regime change" (p. 133). Without knowing how traditional elites fared in other states or in previous regime changes, it is difficult to ascertain whether the rate of survival reported is significant. A second problem concerns the choice of case. Hagopian herself admits that the traditional elites in Minas Gerais enjoyed a special relationship with the military leaders who took charge after the coup in 1964. If so, then we would hardly expect a significant turnover in the state's political leadership. A stronger test of Hagopian's argument would involve studying a state dominated by elites who were less favored by the military. If we take the military at its word—that it was primarily interested in purging Brazilian politics of its corrupt elements—then we would expect to find the demise of traditional politicians throughout Brazil. If by purging the country of corrupt politicians, however, the military really meant removing its opponents from power, then we would expect the elites in Minas Gerais to fare comparatively well. By maintaining a healthy skepticism toward military rhetoric, the expectation that the traditional elites in Minas Gerais would be removed falls apart.

Perhaps more troubling, there is no way to falsify Hagopian's argument that traditional politics in Brazil is as strong as, or even stronger than, before the authoritarian period. Two aspects to this problem bear noting. Inherent in any study concerning "traditional" politics is the problem of knowing exactly what it is. Hagopian's analysis operates at a level which makes distinguishing between pork-barrel politics—a common feature in most democracies—and traditional politics very difficult. For example, to illustrate the rise of state clientelism in Minas Gerais, Hagopian lists a number of government programs in housing, health, and education that were enacted to maintain political support for the military. There can be no doubt, especially in Brazil, that a significant proportion of those resources involved clientelistic arrangements. Allocating additional funds to housing, education, and health does not, however, automatically imply clientelism is at work. It is difficult, even at the state budgetary level, to determine exactly which programs are clientelistic and which actually improve the lives of the state's citizens. To make that determination, studies that carefully examine developments at the local level may be more helpful. Here I am referring to the kind of work by Judith Chubb on southern Italy (*Patronage, Power, and Poverty in Southern Italy*, 1982) and Robert Gay on Rio de Janeiro (*Popular Organization and Democracy in Rio de Janeiro: A Tale of Two Favelas*, 1993).

In addition to the difficulties associated with identifying clientelism at the state and federal level, a more fundamental problem with falsifying evidence exists. In her introduction, Hagopian concedes that traditional politics may not last indefinitely: "Although democracy's early years have not been promising, the conclusions of this study may be revised by political events" (p. 35). The problem is that we do not know what political events to look for. Do the mass demonstrations calling for Collor's impeachment imply traditional politics are on the decline, or was his ability to perpetrate so much corruption a signal that traditional, clientelistic policies are still strong? Does the privatization of the large state-owned mining concern (Companhia Vale do Rio Doce) suggest state clientelism is a thing of the past, or do the numerous court injunctions lodged against its sale show traditional elites are not ready to jettison an important political resource? When, exactly, will we know that traditional politics are on the decline?

Hagopian's balanced and accurate portrayal of traditional politics in Brazil permits an alternative interpretation of the evidence. Despite her explicit rejection of institutionalist explanations, a tension exists throughout the book that belies an inability to embrace fully agent-centered arguments. In several passages, Hagopian refers to the threat that a more competitive electoral process posed to the traditional elites. It was the independently organized rural peasants who captured enough influence in the Brazilian Labor Party (Partido Trabalhista Brasileiro) that threatened the traditional elites' survival, causing them to support the 1964 coup. Referring to the postauthoritarian period, Hagopian herself admits that open political competition may erode the power of the traditional elites. If so, an interesting hypothesis begins to emerge: Genuine political competition may serve as the most effective means to diminish the dominance of traditional political elites. Authoritarian Brazil gave new hope to the traditional elites. Without the backing of exclusionary practices enforced by the military, the days of clientelism may be numbered. Indeed, institutions may matter.

The book is important for several reasons. First, it provides a comprehensive and detailed account of politics in an important Brazilian state, explaining how politicians learned to cope with and respond to institutional changes they helped initiate but over which they quickly lost control. Most important, it addresses head-on a key question that confronts comparativists generally and Latin Americanists specifically: Can democratization by itself address the problems of poverty, income inequality, and social justice that plague so many newly democratized nations? For this reason alone Hagopian's *Traditional Politics and Regime Change in Brazil* should be read by anyone interested in democracy, development, and the interaction between politicians and the institutions they inhabit.

Daughters of Palestine. By Amal Kawar. Albany: State University of New York Press, 1996. 159p. $49.50 cloth, $16.95 paper.

Gender Bias and the State: Symbolic Reform at Work in Fifth Republic France. By Amy G. Mazur. Pittsburgh: University of Pittsburgh Press, 1995. 312p. $49.95 cloth, $22.95 paper.

Comparative State Feminism. By Dorothy McBride Stetson and Amy G. Mazur. Newbury Park, CA: Sage, 1995. 334p. $52.00 cloth, $24.95 paper.

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These three books deal with aspects of women's political leadership in comparative political perspective. The Mazur book on France and the volume edited by Stetson and Mazur focus primarily on state structures aimed at eradicating gender-based inequality, while the Kawar book is a study of women's roles in a radical political movement, the Palestinian National Movement.

There is a close theoretical and empirical connection between the Mazur study of French equal rights and the comparative work edited by Stetson and Mazur, which includes essays on equality policies in many different European nations. Mazur's analysis focuses on the essential failure of French policy to address issues of systemic inequality. From the 1960s, beginning with de Gaulle, to Pompidou and Giscard and on to Mitterand in the 1980s, the analysis emphasizes the triumph of symbolic reform over material change. The *egalité professionelle* (or equal employment) policy created the image of change but not the reality. The study focuses on three legislative enactments: the 1972 Equal Pay Law, the 1975 Equal Treatment Law, and the 1983 Egalité Professionelle Law. For male policymakers, supporting symbolic equality reform was a way to woo voters interested in women's rights issues. Symbolic reform also was a way to develop connections between evolving European Community policy in this area, at relatively low domestic cost. Mazur's book analyzes the failures of legislation and executive leadership to formulate policy adequately as well as the weakness of implementation and commitment. She demonstrates that, by the time the three policies examined were enacted, provisions for authoritative measures to promote gender-based equality were abandoned, essentially vitiating the possibility for concrete steps to eradicate gender-based inequality. Government officials employed strategies relying on nondecision making and stalling to forestall meaningful policy. In the main, the laws passed neither were the result of an active policy network of groups involved nor, given their weakness, would they be expected to create new networks based on group "identity, goals and capabilities" (p. 5).

The political situation regarding equality policy in France was complicated by what Mazur describes as a weak labor movement and a women's/feminist movement that frequently was divided and without a national presence. Only the struggle for abortion reform, as opposed to that for equal rights, was able to create a national French women's movement advocating for changes in public policy; the new women's movement remained disinterested in reformist employment-related equality. In contrast, the greater success of equality policy enactment and enforcement in the United States and United Kingdom is attributable to more active policy networks in those countries. Ultimately, Mazur concludes that the French approach was weakened by the stronghold of bureaucratic dominance, the inability to litigate on behalf of women's interests, and the absence of a strong feminist policy network.

More systematic attention to the role of corporatism in limiting political possibilities for promotional groups would help to clarify the issue of whether this policy arena was unique or whether other change-related policies would be likely to meet a similar fate. Mazur's in-depth case study of the limitations of equal employment policy in France does provide important documentation and analysis regarding symbolic reform over several decades.

Stetson and Mazur's *Comparative Political Feminism* extends the analysis of gender-related policy to institutionalized feminism or women's policy machinery in fourteen nations in Europe and North America as well as Australia. Essays in the edited volume describe the range and diversity of state structures dedicated to promoting women's rights and roles in advanced industrial countries and analyze the extent to which they achieve feminist goals. The authors also draw some conclusions about the combination of national differences and the establishment of women's policy machinery, women's movements, and policy change within each country studied.

Among the more successful cases documented in this analysis are Canada, Australia, and Norway. The term "femocrat" was invented in Australia in the 1970s to describe the phenomenon of feminist advocacy within government, supported by movement pressure from the outside. Marian Sawer's article describes the relationship between the feminist Women's Electoral Lobby and the Office of the Status of Women. The Women's Budget Statement helped to mainstream issues of equality through its gender-based audit of all federal departments and agencies. In Canada as well, a coalition of many feminist groups, the National Action Committee, worked to establish and promote policy development in the Canadian Advisory Council on the Status of Women. In both countries, over time and with the advent of more conservative regimes, funding and focus have changed, both within government and the feminist movement. It will be interesting to continue to examine the fate of these relatively pathbreaking efforts in years to come.

Similarly, the Norwegian Equal Status Council has worked closely with the activist women's movement to achieve significant change. It is interesting to contrast the significant progress of the Norwegian movement with those in Denmark and Sweden, suggesting that all Nordic nations are not the same. In Denmark, according to author Anette Borchorst, the effect of the Equal Status Council has been limited by the dominance of the "social partnership" model. The primacy of economically based members in the corporatist system restricts the radical potential of equality policy, while at the same time assuring consensus for those policies and activities that the government endorses. Elman's article on the equality ombudsman in Sweden documents an even weaker role for state policy machinery. In the absence of an extensive autonomous feminist movement and the weak power accorded to the state equality policy machinery, limited effect has occurred. Equality policy has been defined primarily in labor-related terms. This excellent article describes the ways in which the narrow policy parameters have defined sex discrimination in terms of economic inequality, consequently making issues related to violence against women, including sexual harassment and battering, more difficult to resolve.

Two interesting articles deal with Britain and Ireland. Lovenduski describes the evolution of policy machinery in Britain, suggesting increased and evolving relationships between women's organizations and more formal government bodies even under Thatcherism. Mahon's article on Irish policy machinery demonstrates that despite continued Catholic hegemony, which has limited women's ability to alter the policy discourse on such issues as abortion and divorce, positive changes have occurred. Through the efforts of liberal feminists and the establishment of new policy machinery, reforms in domestic violence and martial rape as well as sex discrimination in education, work, and the media have been placed on the agenda, and some new policies have been implemented. Stetson's article on the United States focuses exclusively on the Women's Bureau of the Department of Labor. While the analysis of this agency, which long predates the advent of the second wave of feminism, is interesting, it is puzzling that the article does not include discussion of other state agencies that deal with equality policy, in particular the Equal Employment Opportunities Commission. The occasionally uniquely powerful role, often complex and problematic as well, played by American bureaucrats and judges, referred to in the Swedish context by Elman, is curiously omitted here. Two nations in which efforts at change have been even more problematic than elsewhere, in part due to late democratization, Poland and Spain, are also included in the national case studies.

This volume of collected essays is a welcome addition to the literature on public policy and comparative politics. The editors and authors draw several conclusions that are worth emphasizing. They include the importance of external international influences, including United Nations activities on behalf of women and the effect of the European Community on member nations. They also emphasize the importance of autonomous feminist and women's advocacy organizations in creating change. The editors and case study authors analyze policy access and influence, comparing differing approaches to empowerment for women. Through the intermediary of state machinery, policy outputs from grants to new policy changes are examined, and the variables responsible for contrasting outcomes are analyzed. Further exploration of variables related to strong versus weak states, party systems, corporatist/pluralist policy approaches, and social democratic versus center/right governments will aid in future studies, as will cross-national analysis. The continued importance of defending women's policy machinery in an era of privatization and continued efforts to marginalize issues related to gender equity is a major finding of this fine collection.

The final book to be considered, Kawar's *Daughters of Palestine*, deals with women's movements, policies, and positions at a different stage of the political process and in a different culture: women leaders in the Palestinian National Movement. The analysis is based on 34 interviews with leaders of General Union of Palestinian Women and other top-ranking women representing important factions of the Palestine Liberation Organization (PLO), a majority of whom served in the Palestine National Council, the PLO legislative branch. The period of activism covered ranges from the revolutionary period of the mid-1960s to the Palestinian/Israeli peace process of the early 1990s. The book covers three generations of women leaders, with the primary emphasis on the second and third generations, who were responsible for the major grassroots mobilization of women. Most of the interviewees are from politically involved, secular backgrounds, although a major theme of the book is to document the tension between traditional values, nationalism, and the revolutionary spirit. Most of the women whose lives and stories are documented emerged from "the special situation of women" emphasizing private and familial obligations and restricting access to public life. While about one-third began their political activism due to family connections, the process of transition to the public sphere for these women took many different paths. Many women also revealed stories of imprisonment and personal sacrifice. The book documents the trajectory of social and political experience traveled by some, from charitable work to advocacy and armed resistance. In a number of instances women's groups dedicated to literacy and vocational training and kindergarten education or to tasks delegated by male leaders were transformed into laboratories for women's mobilization, as has been the case in other cultures. To some extent, the fortunes of women activists mirrored those of the PLO and the Palestinian movement and the factions in which they were involved.

Today, women in the factions of the PLO occupy only a limited number of policy positions; they never became a critical mass in the PLO leadership ranks, although individuals such as Laila Khaled, Hanan Ashrawi, and Laila Shahid may represent a new breed of leaders. At the same time, because the strength of many women's organizations lay in the diaspora-based women's committees, the formation of a Palestinian state ironically presents new challenges to their continued importance.

One conclusion of Kawar's book is rather sobering, as she suggests that Palestinian women's movements are becoming somewhat marginalized in the 1990s, with a decline in mobilization by grassroots women's committees. In 1994, however, women's leadership in the West Bank held a path-breaking press conference at which they called attention for the first time to the role of women's participation in the liberation struggle and the need to address specifically women's concerns in the new stage of state formation. The meeting was intended to demonstrate the unity of Palestinian women's organizations behind a women's rights agenda. This was an initial effort to create an autonomous women's movement representing different factions, rooted in the activism of the West Bank and Gaza Strip. For the first time, women's committees are devoted to a women's rights agenda, rather than simply social service activity, as well as political education to foster women's citizenship. Kawar suggests that in addition to patriarchal obstacles to achievement of their newly ambitious agenda, Palestinian women will have to cope with the new challenges to their goals posed by the upsurge of Islam.

Kawar has presented important documentation of the relationship between national liberation and women's liberation. Yet, this is a largely descriptive account, which does not attempt theoretical or comparative political analysis. Nonetheless, many of the themes discussed in the other volumes under review are also reflected here: the effect of international feminism, connections between women's policy influence and leftist and traditional political forces as well as party factions, and symbolic reform related to women's policy concerns. Together, and singly, these works contribute to the growing literature on women movements and policy change in comparative perspective.

Comparing Policy Networks: Labor Politics in the U.S., Germany, and Japan. By David Knoke, Franz Urban Pappi, Jeffrey Broadbent, and Yutaka Tsujinaka. Cambridge: Cambridge University Press. 288p. $54.95 cloth, $17.95 paper.

Robert Huckfeldt, *Indiana University*

Knoke, Pappi, Broadbent, and Tsujinaka undertake an important and ambitious effort to compare labor policymaking across three national contexts—Japan, Germany, and the United States. Even in the context of modest goals, such an undertaking would constitute a significant intellectual challenge, but the authors are not content with a superficial description of the policy process, or with a highly abstract conception of individual actors and their idiosyncratically defined interests, or with blow-by-blow stories of policymaking intrigues. Rather, their intent is to locate the production of labor policy within a constellation of intersecting actors, inside and outside government policymaking institutions. Their argument is that policy is produced as the consequence of structured patterns of social interaction between and among significant political actors. The form and structure of these interaction patterns is, in turn, a consequence of formal aspects of institutional design, cross-national variations in the extent to which the boundaries between state and civil society are permeable, the exercise of political interest within policy domains, and the particular configuration of information networks and resource exchange networks.

The authors adopt an "organizational state perspective" to guide their analysis. This framework treats formal organizations as unitary actors, and elite individuals are thus relevant only as agents of these collectivities. Such a vantage point directs attention away from a theory of political organization and toward an organizational theory of politics. In other words, the effort in *Comparing Policy Networks* is not directed toward the patterns of interaction and exchange within organizations, but toward the pattern of interaction and exchange *among* organizations. Such a focus will be less than satisfying to some students of the policy process, but it allows the authors to confront a series of important and enduring issues regarding the functioning of democratic politics.

None of these issues is more important than the roles played by governmental and nongovernmental actors in coordinating and consolidating the policy process. In particular, what is the structure of opposition and conflict within that process, and how does it vary across differently configured democratic political systems? At one extreme, the opposition may be totally separate and apart within the policy process, making negotiation and accommodation nearly impossible. Alternatively, to the extent that the organization of interaction and exchange between and among the advocates and the opposition is more diffuse, opportunities for accommodation and bargaining are likely to increase. If advocate and opposition groups *are* integrated within the policy process, then which actors play important integrative roles, and how directly are the various groups tied together?

These are certainly among the most enduring political questions within policy research. One has only to recall Robert Dahl's (1960) discussions of Mayor Lee and his crucial coordinating role in the New Haven political environment of *Who Governs?* to regain the silver thread that links this research to a long tradition of intellectual concern regarding the real-world functioning of democratic politics. Indeed, in their recent work on the *Hollow Core* (1993), John Heinz and his colleagues argue that the integrative function is largely absent among groups affected by the process, thereby suggesting that such roles are the sole domain of public officials likely to play the crucial roles of integration, coordination, bargaining, and the organization of accommodation. In contrast, the analysis of *Comparing Policy Networks* diverges with respect to the existence of a hollow core, arguing instead that peak organizations are centrally located to play these roles within the informal structures of communication and influence, even though their particular locations vary across networks of communication and resource exchange.

These are only some of the issues addressed in *Comparing Policy Networks*. Such an analysis must necessarily take place at multiple levels of organization, and one of the particular contributions of this effort is that it places such a multilevel analysis within three different political systems, thereby adding yet another level to the analysis. Regardless of the fact that these systems are frequently viewed as being radically divergent and difficult to compare, the authors show that their analytic framework travels quite well across the national boundaries. Indeed, the multiple levels of analysis employed by the authors allow them to demonstrate quite clearly many of the similarities and differences across the three nations.

Moreover, *Comparing Policy Networks* demonstrates that institutional design, self-interest, and political purpose are not incompatible with a focus on the informal structure of political interaction. As the authors show, self-interest is realized within a particular setting that is characterized *both* by informal networks of information and exchange *and* by the particular design of formal political institutions. Hence, the analysis of political strategy and political choice often becomes more revealing when it incorporates contingencies based on both formal and informal aspects of political organization. Furthermore, these twin aspects of political organization evolve in response to each other. Informal arrangements arise in response to formal institutions, and institutional design responds to the circumstances of informal social organization.

In summary, this is an important book that addresses issues of compelling interest to students of democratic politics. Moreover, it sets a higher standard for comparative analyses by demonstrating the possibilities for comparison among and between political systems. *Comparing Policy Networks* is not light reading, but it is well worth the intellectual investment required on the part of the serious reader, and it points toward future lines of productive research both in comparative politics and in policy research.

The Politics of Clientelism: Democracy and the State in Colombia. By John D. Martz. New Brunswick, NJ: Transaction Publishers, 1997. 358p. $44.95.

J. Mark Ruhl, *Dickinson College*

John Martz is a leading authority on Latin American politics who has been studying Colombia and its neighbors, Venezuela and Ecuador, for four decades. In 1962 he published *Colombia: A Contemporary Political Survey*, which examined the nation's midcentury political collapse into *La Violencia* and military rule and described the early years of political stabilization under the National Front, an experiment in institutionalized coalition government. Martz's latest book essentially begins where the earlier volume ended and analyzes the development of Colombian politics through the entire National Front era and after (1958–95). His central argument is that patron-client relationships are still crucial to understanding the maintenance of elite control in Colombian politics, but that the forms of clientelism have changed over time, particularly as a consequence of urbanization and the development of a modernizing bureaucratic state. In recent decades, the Colombian state has replaced Liberal and Conservative national political party leaders as the principal source of patronage and other material rewards. In Martz's terms, Colombian clientelism has become bureaucratic and corporate rather than patrimonial but, nonetheless, is likely to remain a central feature of the nation's politics for many years to come.

The book is divided into three parts. Part One discusses the clientelism concept in depth and demonstrates its relevance to the politics of Latin America in general and Colombia in particular. It also provides some historical background on the pre–National Front period Martz ably analyzed in his 1962 volume. Parts Two and Three discuss in turn each of the Colombian administrations since 1958, using a common organizational format that focuses attention on the national environment, the policies of the administration, social and political controls, party competition, candidate selection, and campaigns and elections. Part Three also contains a concluding section that assesses the status of Colombian clientelism in the 1990s.

The Politics of Clientelism offers a valuable political history of an important period in Colombia. Martz has an excellent command of the literature on Colombian politics in both English and Spanish and has spent considerable time doing research in the country. His well-documented analysis is balanced in its assessments and consistent with mainstream interpretations of events. His discussions of the post-1958 administrations are much more thorough and detailed than those available in other recent English-language studies, such as Robert Dix's *The Politics of Colombia* (1987), Jonathan Hartlyn's *The Politics of Coalition Rule in Colombia* (1988), and Harvey Kline's *Colombia: Democracy Under Assault* (1995). As such, the book will be most valuable for specialists and for graduate students preparing to do research on the country. Martz is especially good at describing the essential style of each administration and the political factors that influenced its performance. He also makes sense of the complex evolution of political factionalism within the two traditional parties over four decades without going into unnecessary detail. General readers who are not as interested in the specifics may find Martz's treatment too detailed for their needs. They also may want more basic information on Colombia's social and economic context than this political study provides. If so, they are probably better advised to read Kline's brief but well-written book. The Dix and Hartlyn studies are also very good but are becoming dated.

Martz's book is a useful addition to the still limited English-language literature on Colombian politics, but it is not without its shortcomings. The introductory segment on clientelism is rather long and is not well integrated with the discussions of individual administrations. The organizational framework also seems to prevent the author from asking some important general questions about Colombian politics. For example, the activities of the Fuerzas Armadas Revolucionarias Colombians (FARC), the Ejército de Liberación Nacional (ELN), and other guerrilla groups are discussed in the context of each presidential term, but the fundamental question of why the nation's guerrilla wars have lasted for so many decades when such conflicts have ended in most of Latin America is not really addressed. Indeed, there is little attempt to place the Colombian experience within a broader regional perspective. This is unfortunate because, as one of the region's oldest formal democracies, Colombia is well worth examining for clues about what the future may hold for the rest of Latin America, now that nearly all the countries in the region have popularly elected governments.

Martz's analysis of Colombia's troubled democratic system should encourage scholars to restrain their optimism about democracy's future in Latin America. Colombia has held regular competitive elections since 1958, and its civilian leaders have, for the most part, retained control over the armed forces during this period. This represents a significant improvement over the violent partisan conflict and military dictatorship Martz encountered when he first visited Colombia in the mid-1950s. During this forty-year "democratic" period, however, the country has experienced an unending guerrilla war; the rise of powerful drug cartels, which have penetrated and destabilized the political system; and some of the highest levels of street crime in the world. The heavy-handed responses of Colombia's security forces to these problems have led Amnesty International and Americas Watch to classify them as some of the worst abusers of human rights in the Americas. Moreover, although every one of the presidents Martz discusses promised to reduce the socioeconomic inequalities that leave a majority of Colombians in poverty, the huge gaps between social classes remain largely intact. It is little wonder that most Colombians choose to ignore the electoral system and its competing party and factional elites; abstention rates of 70% of eligible voters are common.

Colombia may have successfully maintained a democracy in a formal sense, but Martz makes quite clear that it is still a very exclusionary and unstable one. Will fifty years of democracy mean something similar for the rest of Latin America? It would be interesting to know to what extent Martz expects to see Colombia's problems repeated elsewhere. Analysts already have highlighted many of the same defects in the new and reconstructed democracies in the region as Martz observes in Colombia. The case he describes so carefully is disturbing precisely because it suggests that low-quality, unconsolidated democratic (or more accurately semidemocratic) regimes may endure indefinitely.

The Policy-Making Process in Contemporary Japan. By Minoru Nakano. Translated by Jeremy Scott. New York: St. Martin's Press, Inc. 1997. 257p. $65.00.

Japanese Democracy: Power, Coordination, and Performance. By Bradley Richardson. New Haven, CT: Yale University Press, 1997. 336p. $35.00.

T. J. Pempel, *University of Washington*

Throughout the 1990s, a number of longstanding verities about Japanese politics and economics ran headlong into new and competing realities. Nearly four decades of exceptional economic performance gave way to collapses in stocks and land prices; corporate failures in banking, insurance, and high tech; and slow to no growth. Thirty-eight years of rule by the conservative Liberal Democratic Party (LDP) ended in 1993 amid revelations of the sleazy but structural links among banks, brokerages, politicians, and organized crime. A period of massive party reorganization followed. Japanese isolationism and domestic polarization over foreign policy withered as the Japanese government dispatched troops for United Nations peacekeeping missions and as socialist prime minister Murayama Tomiichi rejected virtually all his party's traditional policies on security and defense in favor of those of his party's longstanding nemesis, the LDP.

Not surprisingly, such events catalyzed considerable debate about whether Japan is undergoing some form of sea change or has merely hit a few unfortunate potholes in its travels along largely familiar paths. Students of Japanese politics, like those who focus on the politics of any single country, must regularly assess the importance of such complex and evolving events. Broadly speaking, any one of three responses is possible. The first is to underscore ongoing national continuities that outweigh the myriad twists and turns of day-to-day politics. Richardson's book leans largely toward this approach. Rich in details and specifics, it musters most of them as evidence of a single pattern of Japanese politics seen as both democratic and pluralistic.

Rather a different response involves underscoring the complexity and detail of discreet events. Such studies may prefigure broader generalizations about the country, some particular subset of policy areas, particular political institutions, or the like; more often the emphasis is on the inherent complexity and myriad interactions involved in each set of events. Nakano's book falls into this category. It opts for more detail and fewer cases, and it makes no effort to isolate any single pattern for "Japanese politics." Rather, microlevel analysis is used to adduce a variety of complex patterns in Japanese policymaking. Like Richardson, Nakano concludes that most of these involve decentralization and pluralism, although, unlike Richardson, he is reluctant to conclude (for reasons never given) that they are democratic (p. 9).

In between these two approaches are those that stress relatively systematic changes over time, emphasizing either linear evolution or the transformative movement from one pattern of politics toward another, from authoritarianism to democracy, for example, or from corporatism to pluralism. Neither book makes any such systematic argument about changes in the overall pattern of Japanese politics, and neither sees the 1990s as representing any sharp break from the past. The Nakano book is the translation of a 1992 publication whose specific cases were drawn almost exclusively from the 1980s. Richardson devotes a chapter to the specific shifts and changes that presage the defeat of the LDP in 1993. But this material neither drives nor informs the bulk of the book's analysis. Given their particular foci, both authors would almost surely see events of the 1990s less as challenges to their major conclusions and more as confirmations. For Richardson, party realignment would further illustrate Japan's inherent pluralism and democracy; for Nakano, recent shifts in policymaking would simply be additional evidence of Japan's underlying political complexity and the desirability of more microlevel analysis. Consequently, readers interested in recent events in Japan, either in themselves or as possibly involving a major political transformation, must be prepared to accept both books as concerned with quite separate problems at different levels of analysis.

Richardson concentrates almost exclusively on national politics. His major concern is with the linkages among citizens, political parties, interest groups, and governmental institutions. Individual chapters examine electoral behavior, political parties, bureaucracy, the legislature, interest groups, and governmental links to the economy. Most offer extensive quantitative data and/or a detailing of numerous cases drawn from newspaper archives. Each chapter addresses one or more important intra-Japan questions, but the overarching conclusion to which all chapters lead is that Japanese politics is horizontal and fragmented rather than vertical and integrated, far more pluralistic than elitist or corporatist. The concluding chapter sums up Richardson's underlying argument: Japan is a "bargained, distributive democracy."

Nakano's concern is to map various patterns of policymaking influence in Japan. Unlike Richardson, he also examines prefectural and municipal politics. The empirical heart of the book involves four 1980s cases: the 1985 reform of the pensions program; the initially unsuccessful efforts to introduce a consumption tax; prefectural budget making; and the relocation of Tokyo's municipal government offices. The author shares many of Richardson's broad conclusions regarding the complex lines along which conflicts develop. Yet, he is reluctant to draw macrosystemic conclusions. Instead, in chapter 2, he offers an elaborate taxonomy of what he sees as six fundamental patterns of Japanese policymaking, most with subtypes. These range across the area enclosed by a two-dimensional matrix, one dimension measuring the cooperative versus the competitive character of the participants' relations, the other measuring the openness and closedness of the particular policymaking for the case at hand. He concludes that some patterns are highly elitist, closed, and government led; others involve cross-party cooperation; some are narrowly clientelistic; others are driven by outbursts of public opinion. In short, for Nakano different patterns emerge around different issues at various times.

Specialists in Japanese politics will gain a variety of insights from both books. Nakano provides a wealth of new empirical material, at least in English, and his analysis of the multiple patterns of policymaking is untypically capable of accommodating various episodes of elitism and pluralism. At the same time, each case exists largely on its own. Although these yield multiple specific conclusions on details, one is always left wondering about the typicality of each case and the conclusions that might be drawn from them collectively. Particularly lacking are comparative reference points by which to assess the Japanese experience despite the author's call for analyzing Japan in the context of the other advanced democracies.

Richardson's book is especially rich in its assessments of voting behavior, party reorganization, and legislative politics, as well as in the number and frequency of what might be termed "throw-away insights," ideas that are refreshingly new even though not pursued in depth. One such example is that Japanese politics suffers from "amoral groupism"—a proclivity toward intense controversy among certain organizations that hold unswerving convictions of their righteousness and resist any search for common ground as treasonous. Similarly useful is his treatment of the importance of the legislative timetable and the ways in which influence over such a technical detail can be more critical to policy outcomes than the balance of party strengths.

Less convincing are several chapters which begin by highlighting controversial interpretations that have arisen about Japanese politics in a comparative context. Typically, these chapters promise to assess the merits of the competing interpretations only to "resolve" the debate by a noncomparatively based conceptual elasticity that permits one favored concept completely to swallow its less-favored opponent. For example, chapter 7 begins by noting that pluralism and corporatism are points on a continuum. But it then proceeds empirically to remove the terms from their comparative reference points and to treat pluralism as anything involving the slightest smattering of conflict, division, or debate. Corporatism meanwhile is equated only with its authoritarian or statist version and occasionally is conflated with elitism and top-down rule. Consequently, corporatism here has little to do with peak associations of interests that have gained governmental legitimacy over specific spheres of policy. Instead, the Japanese farming cooperative association, despite enrolling 99% of Japan's farm families and having extensive control of numerous government programs, is taken as an example of Japanese pluralism because it "engaged in adversarial behavior" with the LDP and the bureaucracy (p. 171). By such a standard, Japan is surely pluralistic; but so, too, are Austria, Sweden, and Norway. Indeed, to the extent that group conflicts arose in Franco's Spain or Salazar's Portugal, these regimes also would be pluralistic. In short, if the standards used to assess Japan were applied to other countries, corporatism would not exist at all. Similar problems arise with the treatment of the "developmental state" and with the influence of organized labor.

In fairness, I should point out that my own work has often led me to conclusions opposite those that Richardson sometimes draws, and I wish Nakano had been more comparatively sensitive. The informed reader can determine whether my observations on these matters are little more than abstractions masking a preference for my own conclusions. But Japanese politics, like the politics of any single country, will make the most comparative sense when assessed with sensitivity to existing theoretical concepts and/or through comparatively informed induction. Such approaches are inherent to making the study of Japanese politics comprehensible and influential within the broader discipline.

Ethnoregional Conflict in Democracies: Mostly Ballots, Rarely Bullets. By Saul Newman. Westport, CT: Greenwood Press, 1996. 296p. $59.95.

Robert J. Thompson, *East Carolina University*

Saul Newman has written an important book for those of us interested in ethnoregional conflict in the democratic political systems of Western Europe and North America. The subtitle of the book accurately indicates the author's concern with the political processes by which ethnoregional conflicts are played out and why, in most instances, these highly charged conflicts are dealt with in a peaceful manner as opposed to the violence of some of the conflicts which dominate the news headlines. He argues that "students of ethnic politics have failed to exploit the modernization process to its fullest. There is a pressing need to focus on the relationship between modernization and the institutionalization, composition and the ideologies of ethnic political movements" (p. 4). He sees modernization as an ongoing process that affects institutions, identities, constituencies, and patterns of political interactions both for ethnoregional groups and majority populations. Thus, he focuses much of his attention not on an original basis for group identity and issues, but on how modernization as an economic and political process has influenced contemporary ethnic identities and issues. Ethnoregional conflicts are evolving and, in most cases, have evolved in ways which can be accommodated within the structure of existing democratic political systems.

Newman does an effective but not overly extensive job of linking his approach to the major literature within the overall field and for each of his five cases. Readers will find his introductory and concluding chapters good starting places for understanding the broader literature, but they should not use them for a complete overview to that theoretical literature or to the cases themselves. Newman is more thorough, however, for the political economy literature relating to ethnoregional conflict in western countries. There is more broadly focused relevant literature that could have been cited without distracting from the tight focus of the author's argument. In fact, it could have made the argument stronger by eliminating questions as to why various works were not cited. This point is mentioned because the book is a significant addition to the field and should not be dismissed by someone who is primarily interested in its theoretical overview. Its theoretical value comes through particularly well in the way the author deals with his case studies.

A major strength of Newman's analysis is his focus on ethnoregional political parties. He categorizes the current parties into four types based on their ideology: neo-traditional, classless-inclusive, selective-protective, and selective-developmental. He then looks at four sets of factors which play (and have played) a significant role in developing the predominant ideological orientation of the parties. Here he considers the ideological and organizational resilience of older ethnic movements; the process of social, economic, and political change that gave rise to the movement; the political environment in which the ethnoregional party chooses to compete; and the processes of re-creating and mobilizing ethnic political identities. He adds to the richness of his approach by viewing the ethnoregional movements and parties as electoral challenges to the dominant political parties and how these respond to that challenge.

Newman uses the ethnoregional conflicts of Scotland, Belgium, Quebec, Northern Ireland, and the Basque region as his case studies. Each of these analyses is a fairly full discussion of the case from the author's perspective, and in each case he makes a convincing argument for his approach. Each is discussed both in historical and contemporary terms, with particular emphasis on the political parties, the changes they have undergone, and the broader systemwide changes within which they have developed. A particularly strong point of each discussion is the author's attempt to connect political behavior data from surveys and elections to his argument. A major problem of the modernization approach to the study of ethnoregional conflicts has been the lack of clear connections between the existing behavioral data and the overall assessments of the effect of modernization on the cases. The focus on modernization is primarily a macro-level approach to the topic, while a focus relying on behavioral data is primarily from a micro-level orientation. Newman does as effective a job of combining these two foci as is available in the ethnic politics literature, particularly considering that he does it for all five cases rather than devote the book to a single case.

An additional strong point is the author's consistent integration of his theoretical arguments, the cases under discussion, and his conclusions. His comparative comments are also useful in helping the reader understand the different ways in which the conflicts have evolved. For example, Newman makes a good point of contrasting the basically peaceful, nonsystem challenging demands of the civil rights movement in Northern Ireland with the independence orientation of the Scottish National Party and the separatist approaches of the Belgian ethnoregional parties, while explicating how violence became the dominant political activity in Northern Ireland. He appropriately notes that the role of religious variation in the cases is important but not sufficient as an explanation. That, in turn, leads him to incorporate the sectarian nature of the conflict and its political institutionalization. At the same time, he contrasts these variations with the Quebec case, which, on the surface, one might expect to be closer to Northern Ireland than Scotland in its patterns.

Overall, this book is an important addition to the research literature on ethnoregional conflicts in democratic system. Readers will benefit from the case studies and the author's theoretical approach. Newman does an effective job of arguing the connections among modernization, ethnic identities, political ideologies, and existing political institutions and practices. There is much that can be gleaned from this work.

The Undermining of the Sandinista Revolution. Edited by Gary Prevost and Harry E. Vanden. New York: St. Martin's Press. 226p. $39.95.

Andrew J. Stein, *Tennessee Technological University*

Nicaragua has been studied as thoroughly as any country, as is demonstrated by the hundreds of publications since the early 1980s. Its attraction was the uniqueness of the transformation during the period of the Sandinista Front (FSLN, 1979–90) and the dramatic, yet inconclusive, changes since the FSLN lost office in the 1990 elections. In this review I will briefly summarize the main arguments, organization, sources, and strengths and weaknesses of the book. Second, I will consider the contribution to the literature from the chapters in the volume. Finally, I will consider some themes and questions that can be culled from this study and reflect on whether the six contributors provide us with material with which to answer them.

Prevost and Vanden's edited volume is a competent synthesis whose goal is to explain why the revolution failed and whether the major gains of the revolutionary period were eroded or reversed by the Chamorro government (1990–97) that followed the FSLN. The Sandinista revolution failed, Prevost notes, because of endogenous factors: the leadership's policy mistakes and inability to sustain redistributive policies, the drain of the Contra War (1981–88), and a movement away from a grassroots focus that was prominent in the first years after the overthrow of Somoza. Exogenous factors consist mainly of the policy of the United States during the Reagan and Bush administrations: CIA sabotage, support for the Contras, and an economic embargo. This book is not the first work on the Sandinista and Chamorro periods, but rather the latest in a series (see Richard L. Harris, "The Nicaraguan Revolution: A Postmortem," *Latin American Research Review* 28(1993):197–213; Richard Stahler-Sholk, "Sandinista Economic and Social Policy: The Mixed Blessings of Hindsight," *Latin American Research Review* 30(1995):235–50; and Shelley A. McConnell, "The Electoral Defeat of the Sandinista Regime," *Latin American Research Review* 31(1996):211–26).

Six areas are highlighted by Prevost as key achievements of the revolution: agrarian reform, social policy (in education and health care), women's rights, autonomous rule for the indigenous peoples of the Atlantic Coast (for the nuances of this issue, see Philip A. Dennis, "The Miskito-Sandinista Conflict in Nicaragua in the 1980s," *Latin American Research Review* 28(1993):214–34), the abolition of a repressive military (Somoza's National Guard, and its substitution by the Sandinista Army; see Oscar René Vargas, ed., *Nicaragua: Gobernabilidad democrática y reconversion militar*, 1996), and the projection of Nicaragua into world politics. Each of these areas is dealt with in a cursory way, though indepth analyses of the six topics by several other scholars have been published. Vanden presents a discussion of models of democracy and why the FSLN moved from a socialist version to a more liberal democratic Western model over the course of the 1980s. Both chapters are based largely on secondary sources and periodicals. In addition, the material presented in the chapters by Prevost and by Pierre La Ramée and Erica Polakoff (on the evolution of mass organizations) has been published in journal articles and edited volume chapters previously.

Richard Stahler-Sholk's "Structural Adjustment and Resistance" is a sound discussion of the FSLN and Chamorro government policies and how they interacted with democratic politics. This chapter addresses more broadly comparative issues: the international context of stabilization schemes, the debt crisis, trade liberalization, and the move from a state-led to a market economy. His chapter and Cynthia Chavez Metoyer's analysis of the role of women in policy design and outputs are both well organized, with tables of data supporting their claims, and are based on extensive nationwide field work (as is the La Ramée and Polakoff entry).

Several themes can better help analysts understand in broader theoretical terms why the Sandinista revolution was "undermined," if one pays greater attention to how domestic political stability is influenced by shifting international contexts. The watershed events of 1989, with the disintegration of the socialist bloc, had a significant effect in both economic and political/military terms for the FSLN. Once the Soviet Union began to collapse, any commitment of Eastern bloc aid or the viability of a socialist economic model were seriously called into question (Stahler-Sholk notes that 30% of GDP was accounted for by state firms, and by 1988 the currency was devalued 60%). Neoliberalism has been a global trend that affected most Third World regimes, revolutionary or not. In addition, while the party–armed forces tie in Nicaragua never approximated that of Cuba, the Sandinista Army suddenly saw its source of weaponry, training, and rationale for its mission seriously threatened. It is worth studying global effects on the revolution other than U.S. opposition, which had a key but not exclusive influence.

Prevost's treatment of the divisions in the FSLN before and after 1990 also suggests a worthwhile subject for comparative politics: the dynamics of the transformation of guerrilla revolutionaries to a competitive political party capable of winning elections. The experiences of cases such as Guatemala, the FMLN in El Salvador, and M-19 in Columbia might lead to fruitful contrasts.

The discussion of women, ethnic minorities, popular organizations, structural adjustment, and the Sandinista party all suggest a theoretical linkage between government decentralization and policy responsiveness. FSLN leaders became "bureaucratic," used decree powers, and hence faced major governability problems, as did the Chamorro administration that followed.

As someone who has studied public opinion in Nicaragua, a final possible contributing factor in the FSLN's demise occurs to this reviewer: the inability of a revolutionary government to transform what the editors term a "traditional" and "authoritarian" political culture over the course of eleven years (especially with regard to the role of religion and attitudes of the private sector and peasantry). The book raises this issue in passing but fails to address it or present evidence with which to evaluate the claim.

Building a Palestinian State: The Incomplete Revolution. By Glenn E. Robinson. Bloomington: Indiana University Press, 1997. 228p. $14.95.

Ian S. Lustick, *University of Pennsylvania*

Although seldom included in comparative treatments of revolution, social movements, or decolonization, the Palestinian revolt of 1987–93, commonly known by its Arabic name as the "Intifada," has been well documented and studied. Dozens of books and articles have established a number of important characteristics of its causes, beginnings, and dynamics. The Intifada resembled many previous eruptions of Palestinian opposition to Israeli occupation, but it was so much more sustained, involved such a huge proportion of the population, and had such a substantial effect on Israeli control of the West Bank and Gaza Strip that it is appropriately addressed as the kind of revolutionary upsurge which in many other countries during this period produced new regimes and even new polities.

As are many of the works that preceded *Building a Palestinian State*, Glenn Robinson's book is substantially based on field research and direct observation conducted during the very throes of the struggle. The distinctiveness of this study, however, lies in the fact that it was prepared as a book not only after the end of the Intifada and after the signing of the Oslo Peace Agreement between Israel and the PLO, but also after Arafat's assumption of the presidency of the Palestinian Authority within portions of the West Bank and Gaza Strip no longer ruled directly by Israel. This affords the author the opportunity not only to place the Intifada within an appropriate historical and regional context—highlighting the relatively late development of an antinotable, nationalist "counterelite" in Palestine—but also to assess its limited success in comparison to similar elites in neighboring countries, to what Israeli governments seem ready to accept as a permanent settlement, and to the aspirations of grassroots Intifada leaders themselves. According to Robinson, the thousands of young, educated, and prison-toughened Palestinians who comprise the emergent elite were seeking to substitute for the Israeli occupation a Palestinian polity embedded within a vigorous civil society, one capable of resisting just the sort of authoritarianism that Arafat and his PLO apparatchiks have been consolidating.

As do most other treatments of the Intifada, Robinson emphasizes the crucial role played by the emergence of a large array of Palestinian self-help organizations in the occupied territories in the early and mid-1980s. In large measure this framework for collective action developed as a function of Israel's ability to prevent or neutralize almost anything else. In the 1980s, however, aggressive right-wing Israeli governments escalated land expropriation and settlement policies and removed the carrot from the carrot-and-stick approach of previous Israeli governments. Some officials were even encouraged to propose a mass population "transfer" that would remove most Palestinians from the West Bank and Gaza. With the old landowning notable elite disempowered and discredited, and the National Guidance Committee of PLO-linked leaders destroyed by Israeli actions, ordinary Palestinians became more open to risky forms of struggle and more willing to rely on and trust the activists who staffed grassroots medical committees, agricultural cooperatives, women's groups, and voluntary work committees.

Meanwhile, different PLO factions organized and competed with each other within all these settings in hundreds of towns, villages, and refugee camps. Leaders of these factions and of the committees and cooperatives themselves were drawn from a large pool of bright, sophisticated, and unemployed Palestinians able to share, on Palestinian university campuses and in Israeli prisons, their sense of oppression and of the ineffectiveness of the PLO "outside." The result was that, once triggered, the Intifada could snowball into a movement with so many different and autonomously organized committees, cells, and focal points, such direct contacts with so many different parts of the Palestinian population, and such large numbers of leaders and potential leaders that Israel could not kill, incapacitate, imprison, or expel enough of the leaders, or intimidate a large enough proportion of the movement's mass base, to bring a quick end to the rebellion.

Robinson's field research is most fully reflected in the chapter on the village of Bayt Sahur, in which popular committees (all outlawed by the Israeli authorities) had enormous success increasing agricultural production and waging an amazingly lengthy and effective tax revolt. Israel tried to make an example of the community by putting it under siege and confiscating huge amounts of movable property. It did bring the village to heel, but not before suffering enormous losses in international and Israeli public opinion. The real point of the story, however, is that the mayor stood, almost alone, against his own village population—opposing the agricultural cooperatives, the tax revolt, and the alternative educational and judicial institutions set up during the Intifada. What Robinson shows is that in the post-Oslo period it was precisely the mayor of Bayt Sahur, and not the heroes of the Intifada, who emerged, with Arafat's direct assistance, as the leading political force in the village.

The reemergence of Bayt Sahur's mayor, who had acted for all intents and purposes as a collaborator during the Intifada, epitomizes Robinson's sharp-edged argument that as much or even more than the Israelis, it is Arafat, the PLO hangers-on he brought with him from Tunis, and the portions of the old notable connected elite he has resuscitated who are responsible for making the Intifada an "incomplete revolution." In other words, according to Robinson, that which no other Arab people has done—create a state bound to a vital civil society and governed by elites constrained to act for more than their narrow self-interest—was within the grasp of the Palestinians, thanks to the sacrifices and accomplishments of the Intifada, but is being thwarted by a "state-building logic" in which the new arrivals are driven to consolidate their own hold on power by displacing the revolutionary elite that brought them back to Palestine.

This analysis is quite consistent with the views of many if not most Intifada leaders, who have been excluded from the decision-making apparatus of the Palestinian Authority and have felt the heavy hand of Arafat's many "security services." It is reflected as well in the recent vote by the frustrated Palestinian Legislative Council recommending that Arafat fire all his cabinet ministers for corruption and incompetence. It is also consistent with the view that the Oslo accords are best understood not as a peace agreement between two peoples but as a deal cut between the Israeli and Palestinian security services to allocate opportunities for profit equitably among Palestinian and Israeli insiders while keeping a lid on Palestinian discontent. Robinson makes his argument even bolder (and more controversial) by extending it to an interpretation of Hamas and escalating Islamic resistance to Israeli occupation as products of a revolt by a new generation of politicized and nationalist-oriented Islamist activists against the old-line leadership of the Muslim Brothers—suggesting important new avenues for research into the long-term implications of Palestinian Islamism.

Reasonable scholars may differ as to whether Robinson has overemphasized internal class and generational conflicts or post-Intifada antagonism between West Bank and Gaza Palestinians and the Arafat-controlled political apparatus. But the argument he presents is a worthy one. It is clear, well documented, and helps debunk culturally essentialist explanations of Arab authoritarianism. Implicit within it is a reminder that no study of Israeli policies can expect to be convincing that does not also consider them as by-products of internal factional, class, and group conflict. If at times Robinson's treatment moves beyond empathy toward apology for some of the excesses of the Intifada "strike forces," overall his book is a fair and sensitive account and contains the best available assessment of the Intifada's political aftermath among Palestinians. An added bonus is that the book is written in an accessible style with enough historical background and contextual explanation to make it ideal as a text for courses in Middle East politics or the politics of revolutions.

Repertoires and Cycles of Collective Action. Edited by Mark Traugott. Durham, NC: Duke University Press, 1995. 250p. $28.95 cloth, $13.95 paper.

Pamela E. Oliver, *University of Wisconsin*

These seven articles on collective action in social movements all work with the concepts of repertoires and cycles of protest. The book grew out of a couple of sessions at the Social Science History Association in 1991, and all but three of the papers were previously published in volume 17 of *Social Science History* in 1993. The format is time-honored. Bring together the originators of important concepts and other researchers who have used those concepts, and if the mix works well, you get a creative focused discussion which advances knowledge and understanding in an area. The concepts of repertoires and cycles are particularly well paired in this kind of exercise, as they have not usually been linked, despite having some important underlying commonalities and interrelations. Charles Tilly started writing about repertoires of collective action in the late 1970s. The key observations he drew from his detailed study of newspaper accounts of unruly events in France and Great Britain in the eighteenth and nineteenth century are, first, that each historical era and locale exhibits a relatively small number of distinct forms of action and, second, that the mix of forms of protest changes through time. Sidney Tarrow started writing about cycles of protest in the early 1980s as an outgrowth of his research on the social movements in Italy from the 1960s onward. Linking his observations of the rise and fall of various forms of action in Italy to much older accounts of the rise and fall of social movements, Tarrow argued that internal dynamics lead protest waves first to rise and then to fall.

For the present enterprise, various scholars were asked to relate the two concepts in the context of their own research. The result does not quite hang together as a coherent whole, but it certainly prompts me to think in some new ways about the changes in movements across time. At least since McAdam's "tactical innovations" work (*Political Process and the Development of Black Insurgency*, 1983), it has been recognized that the upswings in cycles of protest are often sparked by new repertoire shifts. Recent research in the wake of Tarrow's work (e.g., by Hanspeter Kriesi, Ruud Koopmans, Jan Willem Duyvendak, and Marco Guiugni in their 1995 *The Politics of New Social Movements in Western Europe* and various articles) has shown that the mix of forms of protest changes across a cycle of protest, generally beginning with a predominance of disruptive actions. Over time there is bifurcation into large peaceful mass demonstrations and growing movement organizations, on the one hand, and increasingly violent acts by isolated radicals, on the other; these shifts appear to be driven by the interactions of social control authorities and elite donors with protesters.

Although the present pieces do not engage this new research, they complement it. The various authors are moving toward recognizing a more continuous evolution of protest forms within and between protest cycles. Tilly's piece provides some reflection and self-critique on the ways the idea of "repertoire" has been used. He argues that a repertoire needs to be understood as an interaction, as a drama with multiple players which evolves across time as the players interact, testing one another's limits and in the process evolving new action forms. His example is the development in Britain of the public meeting and the demonstration between 1758, when democracy and public gatherings were both suspect and contentious ideas, and 1834, when public meetings were normative and demonstrations had become well-defined forms of events. Tarrow's piece is moving in the same direction of considering the evolution of repertoires. He argues that repertoires do not suddenly change but that their evolution occurs more rapidly in the "moments of madness" at the peak of a protest cycle, as action innovations occur and diffuse more rapidly, becoming part of the "traditional" repertoire of the next generation.

Four of the other essays pursue this same general issue of the evolution of repertoires through cycles of protest. Mark Traugott sketches the history of barricade construction and communal insurrection in Paris neighborhoods from its specific origin in a 1588 event through 1968, rebutting assumptions that such insurrections were peculiar to the revolutionary times between 1795 and 1848, while showing how the barricades that reemerged in 1795 after a 150-year hiatus were different in form and function from their predecessors but had obvious roots in the historical memory of Parisians. Only in 1848 did barricades diffuse to other countries. Marc Steinberg picks up Tilly's idea of repertoires as evolving interactions, describing the discourse and counter-discourse as the Spitalfields silk weavers, initially protected by special laws that maintained their pay and competitive position, responded to the attacks on those laws, and showing that the weavers responded to owners' discourses attacking the laws with language about property rights and free trade by defending those same laws as a laborer's property right. Doug McAdam distinguishes the movements that initiate a protest cycle from spin-off movements that are inspired by the initial movement. He speculates that the initiating movements arise from regime crises or the empowerment of a single challenger, and he says it is unclear whether it is better or worse to be a spin-off movement. Craig Calhoun's essay criticizes the idea of "new social movements" as reacting to a very narrow and historically specific concept of "old" social movements, presenting examples to show that all the elements thought to be defining of "new" social movements were present in the early-nineteenth-century movements in the United States, including a focus on identities (gender, nationalism), defense rather than offense (communes, utopias), politicizing everyday life, nonclass or middle-class mobilization, self-exemplification (living one's principles), unconventional means, and partial and overlapping commitments. He ends with a critique of the ways in which social scientists ignore these themes in protest movements so that they have to be "rediscovered."

The other two essays address rather different themes. James W. White presents quantitative data on social conflict, political conflict, political protest, and economic factors in Japan from 1590–1900. His data show clear evidence of protest cycles, but these seem to be driven generally by economic crises and the general weakening of government, and thus do not fit Tarrow's model well. Charles Brockett addresses the longstanding problem of the relation between repression and protest, in which theory predicts that the state should engage in narrowly targeted repression against protesters in order to limit protest. The question, then, is why repression became widespread and unselective in El Salvador and Guatemala. Brockett's answer is that repression increases the mobilization of the already-organized and those connected to them, but it discourages the unorganized; if a population is not organized, then indiscriminate repression works well at keeping it demobilized. This changes, he says, in the upsurge of a protest cycle, when an opening in political opportunity organizes a lot of people, who are then mobilized by repression, a claim which seems to me to have elements of tautology. Nevertheless, his concern with examining the dynamics of repression in different contexts is important and could be linked with recent European scholarship on this topic.

The essays in this volume are typical of the genre of conference papers with interesting ideas that are not fully developed, loose arguments, and preliminary results or suggestive illustrations rather than conclusive evidence. Taken together, all the essays point to the importance of examining states and social movements in interaction with each other and with the larger international political economy. Read in the context of other research and theory in this area, they contribute to our growing understanding of movement evolution.

Democracy, Capitalism and Empire in Late Victorian Britain, 1885–1910. By E. Spencer Wellhofer. London: Macmillan; New York: St. Martin's Press, 1996. 264p. $59.95.

William H. Field, *Georgian Court College*

It is widely assumed that religion undergirded politics in late-Victorian and Edwardian Britain. The nineteenth-century historian Thomas Macaulay described the Anglican Church as "the Tory Party at prayer," while the Liberal Party came together partly as the outgrowth of Nonconformist objections to the religious supremacy of the Anglican Church. In the light of this dominance, and the party system that sprang from it, the reasons for the rapid rise of the Labour Party in the early twentieth century and the near-total collapse of the Liberals after World War I present a puzzle.

Scholars have grappled with this question repeatedly over the last sixty years. In *The Strange Death of Liberal England,* George Dangerfield (1935) ascribed the Liberal collapse to bickering among party elites and sheer bad luck. Ross McKibbin placed the blame on the doubling of the electorate following the franchise reform of 1918 (C. Matthew, R. McKibbin, and J. Kay, "The Franchise Factor in the Rise of the Labour Party," *English Historical Review*, October 1976, pp. 723–52). Finally, Marxian and Weberian scholars point to socioeconomic change as the source of Liberal troubles and Labour success: As religion lost influence in individual lives and gave way to class as the primary basis of group identification, a class-based party system triumphed over the old religious cleavage.

Democracy, Capitalism and Empire examines each of these hypotheses in light of the interplay between Britain's late-nineteenth-century social structure and political forces. Spencer Wellhofer begins by showing the extent to which the traditional hierarchy that characterized pre-Victorian social structures was being challenged by a variety of class and religious groups. It was clear that Alexander Pope's seventeenth-century view of society as a "vast chain of being" tying individual to individual in a preordained social hierarchy was disappearing, but it was not clear what social structure would replace it. By discussing alternate views of modernity put forth by Karl Marx, Lord Salisbury, William Gladstone, Alfred Marshall, and others, any one of which might have replaced the "vast chain of being," Wellhofer introduces the reader to the political and social forces that were attempting to reshape British society. He then explores the data first presented by Kenneth Wald (1983) in *Crosses on the Ballot.* Using latent variable path analysis (LVPA), Wellhofer introduces a dynamic, quasi time-series element to his analysis such that he is able to uncover changes in electoral support over the eight elections between 1885 and 1910. The conclusions challenge conventional wisdom on the evolution of British politics and offer a comment on the country's transition to democracy.

Wellhofer's three conclusions challenge the findings of all three sets of scholars mentioned above. In contrast to class analysis, Wellhofer concludes that much of the Victorian working class existed outside the structures of organized religion and so did not find Liberal appeals to the Nonconformist conscience compelling. As the Liberals focused more on these appeals, this unchurched working class grew increasingly disenchanted with the party and so was ripe for mobilization by Labour or cooptation by the Conservatives. In a challenge to the franchise argument, Wellhofer finds that both major parties developed viable recruitment strategies in response to the 1885 franchise expansion, but the Conservative appeal to "Establishment, Education, Entertainment, and Empire" proved more attractive to the working classes than Liberal appeals to a nonexistent class solidarity. Wellhofer also downplays the effect of World War I. Although he does not examine electoral or social change after 1910, he finds considerable evidence that the Liberal Party, caught between a rising Labour Party and a restructuring Conservative Party, was in serious electoral trouble well before the war began. Labour's success in capturing working class voters in peripheral Britain, coupled with similar Conservative success in the core and among Catholic voters throughout Britain, proved an insurmountable obstacle to the Liberals.

These findings reinforce and add to other revisionist work on the period. In *Political Change and the Labour Party 1900–1918*, Duncan Tanner (1990) challenged the notion that the electoral mobilization of a homogeneous working class doomed the Liberals. Wellhofer agrees that the working class was more divided than united, but he adds a strong regional component to the mix. While Tanner placed more emphasis on the political situations in individual constituencies, Wellhofer emphasizes rural and core-periphery differences. These, along with the finding that the Liberals were in trouble as early as 1906, represent Wellhofer's main departures from Tanner's argument. This more structured core-periphery framework adds more coherence to the period and allows a general comment on democratic transitions in the context of various alternative coalitions among land, labor, and capital. Traditional analyses of the European transition assume relatively monolithic social classes that enter into national alliances to further or retard democracy (e.g., Barrington Moore, *Social Origins of Dictatorship and Democracy*, 1968, and Gregory M. Luebbert, *Liberalism, Fascism or Social Democracy*, 1991). Wellhofer challenges this logic by relating Britain's successful transition to the fragmented nature of rural Britain. Riven by class, religion, and territory, rural Britons divided their loyalties between the Liberals and Conservatives, precluding thereby the emergence of a fascist iron-and-rye coalition.

While the conclusions are clear, the statistical argument is quite hard to follow for those unfamiliar with LVPA. Even though Wellhofer provides a brief appendix explaining the method and relating it to more familiar methods, readers are advised to develop a familiarity with the statistical tools prior to attempting to understand the evidence and to go slowly through the analytic chapters. The appendix alone is not a sufficient introduction to LVPA. Nevertheless, for country specialists, *Democracy, Capitalism and Empire* deserves to stand alongside the works of Duncan Tanner in providing a revisionist explanation for this turbulent period of British history, and it should be read as well by students of democratic transitions in general.

Corporatism and Comparative Politics: The Other Great "Ism." By Howard J. Wiarda. Armonk, NY: M.E. Sharpe, 1997. 221p. $59.95 cloth, $21.95 paper.

Douglas A. Chalmers, *Columbia University*

For some time now, political scientists have been using "corporatism" to describe state incorporation of social groups in many eras and regions. Almost three decades ago Howard Wiarda was an early contributor to this stream by describing the institutions resulting from distinctive cultural traditions in Latin America as corporatist. The volume under review seeks to provide a guide to the phenomenon, its various meanings, some scholarly contributors, and the trends in corporatist institutions themselves. A rapid overview, it reads a bit like notes for an interesting class, using different perspectives, providing references to research topics, and, once, even admonishing the reader to review earlier chapters.

Wiarda meets the challenge of discussing the cloud of lexical meanings of corporatism by classifying and typologizing. The "things" Wiarda is classifying, however, are of several different kinds. He (appropriately) sometimes uses the term "corporatism" to refer to political ideas, sometimes to institutions, and sometimes to the way social scientists organize their research and theory—which he calls their "approach." It is a bit confusing and not always clear which he is addressing at any one time. I will comment on each.

Wiarda gives a certain prominence to corporatism as an ideology, a set of ideas held by political actors and philosophers, combining norms and beliefs about the world. The subtitle of the book, "The Other Great Ism" (the other two are—not always consistently—Marxism and liberalism), highlights his concern, as does the chapter on "The Corporatist Idea throughout History." He is more interested in the movements espousing them than in elaborating the ideas themselves, however. It was in the century after the French Revolution in continental Europe, he argues, that the corporatist ideology was strongest, rivaling Marxism (or socialism) and liberalism. He has an interesting summary discussion of the entanglement of corporatism and fascism. In those eras, the idea of a third "ism" seems appropriate, but in the last forty years, the division into three streams seems rather more forced, certainly in terms of a conscious set of ideas. He notes that corporatism as an ideology has faded in the last part of the twentieth century, even as the practice grew. He calls the surge of corporatist institutions after World War II by the name of "manifest corporatism," negatively highlighting that constitutional designers avoided the term. Particularly when talking about corporatism in the United States, he emphasizes the controversial nature of corporatist ideology, although he professes to see a revival recently, with labels such as "communalism, solidarism, cooperatism or even ethnic pride" (p. 14).

He talks at greater length about corporatist institutions. Chapters 4–6 offer sketches of their character in many different world settings, including the United States. Rather than provide a definition for what is common to all its manifestations—he considers but rejects Schmitter's much-cited one—Wiarda suggests three guidelines to distinguish corporatist institutions from other types, that is, a strong but not totalitarian state, a limited number of interest groups, and interest groups that are part of the state. In one classification, he identifies four types of corporatism, which emerged in order in Europe and at various times, and mixes in other parts of the world. These are natural, ideological, manifest, and neocorporatism. Natural corporatism is identified with "basic, historic, grassroots organizational units on which early society is structured" (p. 95). These include families, clans, and neighborhood associations. Presumably they become corporatist only when they are explicitly built into the state. Ideological corporatism covers a wide range of conservative and official efforts to incorporate interests preemptively into the state apparatus. Manifest corporatism refers to the arrangements put into place before and after World War II in Europe and to some extent in the United States to manage social conflict. Neocorporatism emerged, he says, in the 1960s and is identified with incorporation of interests in various policy processes.

Applying these various categories in many different countries, Wiarda brings together a wide range of observations about political organization of interests. It is not a simple or altogether consistent scheme of analysis; the definitional boundaries are not always clear. But he conveys the sense of variety.

A recurring preoccupation of the book is with corporatism as an "approach." Wiarda devotes a chapter to this, "Towards a Corporatist Model," and makes numerous references to controversies among scholars, including himself. He implies that there are three great approaches, too, but he is better at drawing distinctions among scholarly emphases within the corporatist school than in contrasting it with others or in explaining what an "approach" is. At various times, he also calls it a model, an analytical framework, and a paradigm. The language throughout most of the book assumes that a corporatism approach is far more than simply noting certain institutions, but later Wiarda says it should be thought of as "an heuristic tool, a teaching device, a means to conceptualize certain social and political trends that other approaches are either silent on or provide inadequate insights to . . . a set of suggestions that alert us to new areas of study" (p. 182). A few lines later, however, he refers to it as "widely accepted as an explanatory device." Rather than decide on one understanding of an "approach," he returns to illustrations of how looking for state incorporation of groups turns up many significant examples.

A chief concern in distinguishing among intellectual trends is highlighting the importance of traditions and culture in thinking about corporatism. To meet criticisms that his earlier work on the "distinct tradition" implied corporatism was unique to Latin America, Wiarda emphasizes corporatist tendencies linked to other traditions, including Confucianism in China and various tribalisms in Africa.

With its many references to different institutions and rich set of classifications, this book could serve as an introduction to the concept. It needs to be supplemented, however, by more analysis before the idea of a third great "ism" can be taken seriously.

Class Counts: Comparative Studies in Class Analysis. By Erik Olin Wright. Cambridge: Cambridge University Press, 1997. 576p. $64.95 cloth, $27.95 paper.

Tom Mayer, *University of Colorado*

The Comparative Class Analysis Project, some results of which are reported in this book, is one of the few truly great social research endeavors of our time. Carried on over a period of 17 years, it sheds more light on existing structures of inequality in advanced capitalist countries than any other research of which I am aware. Through the efforts of Erik Wright and his coworkers, a unified questionnaire about class relations was administered to random samples of labor force participants in more than 15 capitalist countries. The data analyzed in *Class Counts* comes from Australia, Canada, Japan, Norway, Sweden, the United Kingdom, and the United States. All students of social inequality, irrespective of theoretical inclination, are deeply indebted to Wright for the intelligent planning and dogged determination needed to complete this enormous undertaking.

Wright initiated the project to help develop a theoretically coherent and empirically viable Marxist class analysis. He was particularly troubled by the question of how Marxist theory, a system organized around a polarized concept of class, could deal with intermediate locations commonly called the middle class. *Class Counts* is the fifth book Wright has written or coauthored using information and/or ideas from the project. It is a quantitatively oriented book specifically written (1) to persuade non-Marxist social scientists that Marxist class analysis can produce rigorous and interesting empirical result and (2) to convince Marxists that useful results can be obtained through quantitative research methods. As in all of Wright's written work, the prose is vigorous, concise, and steadfastly free from unnecessary jargon. Wright has a remarkable capacity to extract empirically testable propositions from a diverse and often contested theoretical literature. As a consequence, *Class Counts* ranks as one of the most conceptually integrated and theoretically focused research reports I have ever encountered.

The class analysis practiced by Wright is based on the notion of exploitation conceived as an "antagonistic interdependence of material interests." The basis of exploitation is an asymmetric relation in which one group possesses certain productive resources, and another group is excluded from access to them. The material welfare of the former group depends upon the material deprivation of the latter. In a capitalist society the principal form of exploitation derives from ownership of the means of production. This defines three basic classes: capitalists (exploiters), workers (exploited), and petty bourgeois (neither exploiter nor exploited). The class structure of a modern capitalist society is far more complex, however, than this threefold division suggests. Two other dimensions are needed to define capitalist class structure: relationship to authority within production and possession of skills or expertise.

In a previous book, *Classes* (1985), Wright conceptualized authority and expertise as types of property each giving rise to a distinctive form of exploitation. Despite the symmetry of this approach, he no longer finds it satisfactory, and *Class Counts* does not use the concepts of organizational or skill exploitation prominent in the earlier book. This creates certain theoretical problems for an approach founded on the notion of exploitation. What is the conceptual status of authority and expertise, and why are they necessary to define capitalist class structure? Are class locations endowed with high-authority and/or high-expertise exploiters or exploited? If they are not exploiters, what is the source of the high incomes and special privileges associated with these locations? Wright acknowledges the conceptual incompleteness of this book, but he deliberately avoids extended engagement with these troublesome issues. Despite these theoretical shifts, he uses the same twelve-class typology to characterize advanced capitalist class structure and do empirical research as in his 1985 book.

The empirical investigations reported in *Class Counts* are divided into four major parts. Part I concerns transformations of capitalist class structure since 1960 and particularly the expansion of the petty bourgeoisie over the past two decades. Part II deals with the permeability of class boundaries to intergenerational mobility, friendship formation, and marriage. The relation between class and gender is discussed in Part III, while Part IV considers the links between class structure and class consciousness. Ten of the 17 chapters are based on previously published articles, but all have been edited and revised for narrative continuity. This book does not read like a conglomeration of discrete articles. On the contrary, it comes across as a wide-ranging but internally coherent interpretation of contemporary capitalist class structure.

The studies carried out under the comparative project show that the class division based upon ownership of productive means has significant consequences for numerous aspects of capitalist society. Indeed, it proves important in almost every analysis reported in *Class Counts*. Nevertheless, advanced capitalist societies exhibit much variation both in the nature of class relations and the effects of class structure. This book contains more than 130 charts and tables, many of them highly informative and some ingeniously conceived. A review cannot do justice to the depth and variety of material covered, but I shall mention a few specific empirical findings.

Even when narrowly defined, the working class is the largest segment in advanced capitalist societies; if more broadly conceived, then it constitutes a clear majority of the labor force in all countries studied. Contrary to Marxist expectations, however, the American working class has declined in size since 1960, and this decline seems to be accelerating. Also contrary to Marxist expectations, the American petty bourgeoisie has been increasing since about 1980, an expansion not attributable to sectoral shifts in the U.S. economy. In every country studied, women are much more proletarianized than men, and they are conspicuously excluded from class locations involving a combination of expertise and managerial authority.

One of Wright's most original contributions is his Marxist analysis of the permeability of class boundaries. He succeeds both in formulating plausible Marxist hypotheses about permeability and in using loglinear methods to test these hypotheses. Wright considers three class boundaries: between ownership and nonownership of means of production (property boundary), between possession and nonpossession of expertise (expertise boundary), and between exercise and nonexercise of authority (authority boundary). He examines three ways these boundaries could be penetrated—intergenerational mobility, friendship formation, and marriage—and four different capitalist societies: Canada, Norway, Sweden, and the United States. The authority boundary proves most permeable, the property boundary least permeable, and the expertise boundary intermediate between these two. This same permeability order holds for mobility, friendship, and marriage and in all four countries (with a minor variation in the case of Sweden). Moreover, authority-expertise-property is the permeability order predicted by Wright's version of Marxist theory.

In both Sweden and the United States, class has negligible influence on the distribution of housework between men and women. Women of all classes perform the majority of housework whether or not they are in the paid labor force. Women are underrepresented in class locations that exercise authority within all seven capitalist countries studied in *Class Counts*. The gender gap in workplace authority is relatively small in the United States and Australia, relatively large in the Scandinavian countries, and huge in Japan. Although the underrepresentation of women in positions of authority apparently results from discrimination, the glass ceiling hypothesis—that the relative disadvantage of women in securing further advancement increases with movement up the corporate hierarchy—is not supported in most countries studied.

As might be expected, class consciousness is closely associated with class location. The anticapitalism of the working class in Sweden, the United States, and Japan equals or exceeds that of any other class. Conversely, the procapitalist consciousness of the capitalist class in these three countries is not exceeded by any other class. Nevertheless, the relationship between class structure and class consciousness exhibits much variability. Class consciousness is highly polarized in Sweden, where three sharply delineated ideological blocks exist: an anticapitalist block centered on the traditional working class, a procapitalist block including capitalists and expert managers, and an ideologically intermediate block including all class locations that might be considered middle class. Japan, in contrast, shows very little ideological polarization along class lines, mainly because the Japanese capitalist class is less procapitalist than its Swedish or American counterparts. The relationship between class structure and class consciousness in the United States lies somewhere between these extremes, probably closer to Sweden than Japan.

Class Counts demonstrates once again that Wright is a versatile and imaginative analyst of data. One learns a lot about making sensible research decisions by reading the methodological appendices attached to twelve of the chapters in this book. Wright also shows impressive intellectual honesty. He often admits being surprised and befuddled by a result, and he steadfastly calls attention to findings that contradict his own theoretical predilections. He routinely indicates that his own methodological practices may confound the interpretation of results, and he willingly acknowledges when his theoretical formulations are incomplete, possibly inconsistent, or otherwise unsatisfactory. Other social scientists should emulate these practices.

I find much to admire about this important book but also some grounds for criticism. Attentive readers will notice that the data are rather old. The U.S. and Swedish samples were collected in 1980, and the most recent survey in this batch (Japan) was fielded a full decade ago. American and Swedish societies have changed a great deal since 1980. While change does not negate the value of analysis, it renders some of the conclusions obsolete and mandates an effort to locate these particular snapshots of class structure within some larger historical trajectory. Wright does not use all the data seemingly available to him. Fully half the empirical chapters rely exclusively upon U.S. and Swedish data, or upon U.S. data alone. This unexplained limitation renders *Class Counts* more like Wright's earlier *Classes* than it would need to be. One gets the impression of a scholar who has accomplished a Herculean task but still lacks sufficient energy to reach a fully satisfactory conclusion.

Throughout this book Wright struggles to show that Marxist class analysis is different and better than Weberian class analysis as represented by Robert Erikson, Anthony Giddens, and John Goldthorpe. Wright considers the major contrast to be the Marxist emphasis on production relations and exploitation. This is certainly an important distinction, but I do not regard it as the decisive difference between Marxist and Weberian class analysis. The really decisive difference emerges from the Marxist conviction that capitalism is doomed and that class analysis should be used to explain why it is historically condemned and what will replace it. This orientation gives Marxist class analysis an inherently dynamic thrust largely absent in its Weberian counterpart. It is also largely absent in Wright's approach to class analysis.

Despite my own sympathies with analytical Marxism, I find Wright's efforts to vindicate Marxist class analysis vis-à-vis its Weberian rival only marginally successful. Notwithstanding prodigious labor and imposing methodological ingenuity, he has not fundamentally altered our understanding of capitalist class structure. Indeed, I believe Weberian class analysts could have obtained most of his important results. Marxist class concepts were crafted to explain how society changes over time, not to delineate an essentially static stratification system. If the terrain of intellectual combat must be the capacity to describe and analyze such a system, then Marxist class analysis will not outperform, and probably not even match, its Weberian competitor. If the capitalist system is immune to fundamental self-generated structural change, then there seems little point to even the most sophisticated variant of Marxist class analysis.

Markets, States, and Public Policy: Privatization in Britain and France. By Nikolaos Zahariadis. Ann Arbor: University of Michigan Press, 1995. 229p. $39.50.

Anthony Daley, *Connecticut College*

The relationship between politics and markets has changed dramatically in the last generation. Political elites in both advanced and developing countries have for the most part forsaken attempts to control market outcomes through the ownership of productive assets. The thinking that nonmarket criteria—political bargaining, party competition, bureaucratic intrigue—should not influence microeconomic behavior has became the mantra not only of the business press (where one would expect it) but also of important schools of comparative political economy. The demise of communism in Eastern Europe and the Soviet Union only reinforced this ideological dominance.

Nikolaos Zahariadis challenges the thinking that privatization can be explained only by the dominance of ideas. He argues that policy entrepreneurs still have powerful incentives to increase state power despite economists' arguments of relative efficiencies. He asks explicitly why political elites should relinquish power and move state assets to the private sector. This is an important, if frequently neglected, question. While the answer may appear obvious to those already converted, the careful scholar finds that elegant models of social behavior rarely predict the nuts and bolts of political choice.

The author develops a model of public policy to explain why some assets remain in state hands while others are sold off. That model borrows heavily from John Kingdon's work on predecisions in American politics (*Agendas, Alternatives, and Public Policies*, 1984). Zahariadis extends this work to explain both agenda setting and decision making. He posits three policy "streams"—problems, policies, and politics—that flow through organizations and whose convergence during opportune moments create the outcome that is to be explained. Zahariadis suggests that governments decide to privatize state-owned assets when three conditions are fulfilled: policies become available within the relevant policy community (operationalized by what are called "technical feasibility" and "value acceptability"), the government needs the money because of budgetary pressures, and the appropriate parties are in power. He applies that model to case studies of three economic sectors (oil, telecommunications, and railways) in Britain and France.

This is a tidy model that allows the author to explain policy outcomes in six total sectors. He finds that oil was privatized in both countries because the policy streams converged, what he calls "coupling." In contrast, railways remained in the public sector—at least until after the manuscript was finished for the British case—because of technical difficulties in Britain and broad unpopularity in France. Likewise, France Télécom remained state-owned while British Telecom was privatized because the policy process in the former was impeded by its position in the state administration. Instead, the telecommunications market, both for telephony and equipment, was liberalized in France by the introduction of competition.

This book has much to recommend it. It attempts to comprehend the policy process for an ideologically charged issue. It argues cogently against purely deductive theoretical reasoning from the public choice and property rights perspectives to suggest that privatization politics is more complicated, at least partially because interests cannot be assumed. It skirts the thorny issue of "national styles of policy making" (for which exceptions can always be found) and instead examines the details of individual economic sectors. As such, it contributes to a growing literature in comparative political economy that examines subnational variation. It makes a very useful distinction between privatization and liberalization—two terms frequently confused in the literature—by arguing that the former concerns a transfer of ownership while the latter speaks to market form. Then it discusses cases in which liberalization may be a prelude to privatization or a substitute for it.

Unfortunately, the book also suffers from problems. First, the absence of field work deprives the study of important data for the argument. For instance, the author needed to explore in greater depth the dynamics of policy networks in both countries in order to uncover the role of managers and policy intellectuals in promoting and executing privatization. Likewise, the unwillingness to examine key cleavages within both the British Conservatives and the French Gaullists deprives this study of empirical richness. Such data can only be garnered through careful interviewing. Second, the study suffers from a failure to link privatization to industrial policy. For instance, a closer analysis of French industrial policy in the 1980s would uncover a key clue to the facility of later efforts at privatization: Socialist nationalization was accompanied by both production rationalization and financial recapitalization. Thus, state-owned firms were in good shape when the Chirac government in 1986 and the Balladur government in 1993 drew up lists of companies eligible to be transferred out of the public sector. Third, while the author claims to bridge comparative public policy and comparative political economy, he underemphasizes the effects of markets on political decision making. Fourth, the model borders on tautological reasoning. Suggesting that "technical feasibility" and "value acceptability" are proxies for the bubbling up of ideas within the relevant policy community is really only saying that ideas win out both when they are possible and when powerful forces back them. This is hardly illuminating.

This reviewer's reservations about the book have as much to do with the literature on comparative public policy as with this work. The inability to conceptualize economic institutions has plagued historical institutionalism, for instance. More generally, the tendency to prioritize model-building at the expense of explaining political dynamics has been an unfortunate side effect of the partial victory of "theory" over "area studies" in the study of comparative politics. Indeed, such acontextualization diminishes both the endurance of theoretical insights and the capacity to explain the messiness of politics.

Reconfiguring national markets will remain a political job. By recognizing the utility of state-owned assets for policy purposes, the author avoids the self-fulfilling reasoning of much of the literature. By examining the policy process cross-nationally, this book provides a contribution to a contentious issue that remains important to British and French politics.

International Relations

Debt Games: Strategic Interaction in International Debt Rescheduling. By Vinod K. Aggarwal. Cambridge and New York: Cambridge University Press, 1996. 613p. $24.95.

Cédric Dupont, *Graduate Institute of International Studies, Geneva*

The cascade of financial distress that hit most developing countries after the tightening of U.S. monetary policy in 1981 has been a major threat to the international financial and trading order in the 1980s and early 1990s. To defuse the crisis, distressed debtors, overly exposed lenders, and international institutions have repeatedly engaged in often dramatic rounds of bargaining over the rescheduling of loans. What has been the result of these negotiations? In fact, there has been significant variation over time and across cases in the extent to which debtors have undertaken economic adjustment, banks have written down debts, and creditor governments and international organizations have intervened in the bargaining process. The central aim of *Debt Games* is to explain this variation through an impressive theoretical, comparative, and historical analysis spanning 170 years of debt rescheduling involving Latin American countries.

The book goes significantly beyond standard game-theoretic approaches to model international interactions. Most studies simply read actors' preferences off the bargaining outcome, or at best provide only casual empirical evidence for assigned payoffs. Instead, this work provides the reader with an operationalizable game-theoretic model based on a "situational theory" of bargaining. The approach consists of a formal method to derive payoffs for actors depending on their basic goals and their individual situations. Based on a set of coding rules, the analyst simply has to code the three variables that define an actor's situation—debt rescheduling resources, overall capabilities, and coalitional stability. The theory then allows one to derive a game structure for the bilateral interaction between lenders and debtors, as well as some informal insights on how the game might change in the future either through actors' efforts or through intervention by third parties. To solve the games for bargaining outcomes, the only formal skill that a potential user of the model needs is an understanding of the concept of Nash equilibrium to solve static normal form games.

The carefully designed theoretical framework pays off in the empirical work. The author examines 61 cases of debt rescheduling involving Argentina, Brazil, Mexico, and Peru over the last 170 years. He divides this long time period into four epochs defined by overall systemic factors and analyzes variation in outcomes across these different epochs. Overall, the model successfully predicts debt rescheduling outcomes in 85% of the cases. The book also has some interesting policy implications, including the fact that a limited set of structural conditions appears to determine the behavior of actors. Decision makers should therefore not be caught unprepared for the inevitable next crisis of debt rescheduling.

Such an ambitious study involves a series of modeling assumptions and coding rules that are open to challenge. Obviously, empirical coding of discrete variables remains a subjective enterprise, and readers may question some of the 61 cases included in the book. But Aggarwal has the basic point right: His predictions can be falsified, and the burden of proof is on his critics. There are, however, more serious potential questions worth considering. First, whereas the trend in international relations has been toward the analysis of the role of limited information, Aggarwal's games are complete information games. Lenders and debtors do not know what the other one will do, but they always know the structure of the game based on the actors' individual situations. Aggarwal argues that this assumption of complete information is a better approximation of actors' behavior than using incomplete information because, "as we shall see in the empirical cases, actors generally have a good sense of the kinds of actors they face in negotiations" (p. 57). He claims in the concluding chapter that his empirical work provides support for this assertion, but I have difficulty with the assumption that an actor can always be sure of his opponent's domestic coalitional stability, irrespective of the kind of domestic political system. Uncertainty about an opponent's domestic processes tends to be a major source of influence on outcomes of international negotiations. I do not see why this would not apply to debt rescheduling. The author does point to the trade-offs involved in relaxing the condition of complete information. Each actor could be facing eight or more possible types of opponents. To my knowledge no one has ever done something similar. Most of the existing work that tests limited information models either assumes two types of actors or a continuum of types (which would make empirical coding a nightmare).

Second, the book's treatment of change is questionable. In Aggarwal's model, new outcomes in future bargaining interactions may come from change in actors' individual situations owing to their dissatisfaction with bargaining outcomes or because of exogenous shocks. Dissatisfied actors may attempt to manipulate either their own or their opponent's individual situation and use power resources, norms and rules, or allies to achieve their goal. While this is an intriguing argument, the book does not provide any systematic way of applying this intuitive logic to the games beyond estimating a likelihood of change. There is no endogenous treatment of change through the repeated analysis of normal form games, only some indirect assessment through the concept of goodwill, which is discussed at length in a technical appendix.

Third, the modeling of strategic interaction does not include the behavior of third-party organizations or governments. Instead, the author follows a "simple utility maximizing decision rule under certainty" (p. 79) to explain the behavior of these actors. Although he agrees that this is a "significant simplification" (p. 79), Aggarwal gives no substantive justification for this choice.

In sum, the discussion on these three important modeling assumptions boils down to the trade-offs that any scholar has to make between theoretical elegance and empirical robustness. It should be obvious to anyone who has tried to apply game-theoretic models to in-depth empirical studies that Aggarwal needed to make modeling choices. The author is relatively unique in his openness in presenting an autocritique of his approach. The question for a reviewer, then, is to decide whether the choices he made yield a compelling analytical account of debt rescheduling. On this score, *Debt Games* is a tour de force. It is well balanced, with a nice mix of abstract concepts, real-world indicators, and empirical richness that goes beyond "illustrative" case studies. I have no doubt that one could make a different set of trade-offs between theoretical complexity and empirical applicability. But this impressive book should serve as a useful benchmark for years to come. Scholars and graduate students alike will find it to be a valuable source of modeling ideas that goes well beyond the examination of international debt rescheduling.

Security Threatened: Surveying Israeli Opinion on War and Peace. By Asher Arian. New York: Oxford University Press, 1996. 308p. $64.95 cloth, $22.95 paper.

Michael Barnett, *University of Wisconsin-Madison*

More Israelis than ever accept the idea of a Palestinian state, yet more Israelis than ever also favor a more hawkish security policy; more Israelis than ever accept the Oslo Accords, yet more Israelis also view them as a threat to their security. Those looking to sort through the mixed signals of Israeli public opinion on security and the peace process should begin with Asher Arian's *Security Threatened*. An authoritative and exhaustive study by one of the pioneer and premier scholars in the field, the book is sweeping in scope and judicious in its attempt to sift through the changing causes, nuances, and profiles of Israeli opinion on security.

By employing recent advances in survey research and political psychology, Arian addresses and assesses a number of pressing issues concerning the malleable and shifting Israeli public opinion on security and peace. The book is organized topically. Chapter 2 explores the changing relationship between the Israeli population's general sense of security against its perception of an implacable Arab threat. Chapter 3 offers a series of observations concerning Israeli perception of peace and its likelihood. Chapter 4 examines the changing security environment of the 1980s and 1990s and its effect on Israeli public opinion. Chapter 5 examines the relationship between public opinion and policymaking. Chapter 6 explores the source of Israelis' confidence in their survival given their perception of a high-threat environment. Chapter 7 employs prospect theory to examine the willingness to take risks for peace. Chapter 8 explores value hierarchies such as democracy, a Jewish state, and security; their tensions and trade-offs; and how different political parties bundle these hierarchies in different ways. Chapter 9 surveys the relationship among opinion, security, and democracy. The concluding chapter explores some central tenets and myths concerning Israeli public opinion on security.

A real strength of the book is its systematic evaluation of the complex and sometimes contradictory relationship among different demographic groups, political parties, and opinions on security issues. Some common observations, such as lower socioeconomic groups tend to be more hawkish than those better off, are largely confirmed; but even here Arian's determination to go beyond the usually suspected relationships leads to some unexpected findings. The historical breadth of the study (1962–94) enables him to track some fascinating developments in Israeli public opinion against the background of wars, changing governments, and ongoing security crises.

Although Israel is well known for its strong cleavages, Arian isolates three features of Israeli public opinion that are common across and sometimes unify the ranks. First, the Israeli public distinguishes between security and peace. Security is a short-term issue that involves the provision of safety and well-being from immediate threats. Peace is a long-term issue that requires an agreement between Israel and its Arab enemies to cease hostilities. On security issues Israelis are fairly militant and have become increasingly disposed toward policies that deal harshly with terrorism and disturbances in the territories. On peace issues, they have become more conciliatory and more accepting of a possible Palestinian state. Although the book concludes in 1994, Arian's observations help explain why many Israelis who supported the peace process nevertheless voted for Netanyahu, who promised security in the 1995 Israeli election.

Second, there has been an across-the-board swing by the Israeli public toward conciliation. While background variables such as education, age, and religiosity help predict the general orientation toward the peace process, a general shift has occurred. This development is largely attributable to the Intifada and the Gulf War; both events caused Israelis to lose faith that the territories could deliver Israel's long-term security and to contemplate the once unthinkable possibility of a Palestinian state.

A third common feature is what Arian calls the "People Apart Syndrome," that is, Israelis feel they dwell in a high-threat environment yet are confident they will overcome those threats. To explain such enduring confidence amid constant menace, Arian identifies a belief system that combines the traits of perceived success (have persevered in the past), denial (no choice), differentiation ("good" versus "bad" Arabs), and the People Apart Syndrome. But the syndrome, which most Israelis share in some part, is the principal way they are able to overcome their anxieties regarding security. The syndrome has two elements: "God-and-us" refers to the mystical and religious relationship involving God, the Jewish people, and perseverance; "go-it-alone" suggests that the world is against the Jewish state. This belief system helps explain continued optimism in an environment that is chronically ominous; the persistence of a sense of community; and attitudes toward the peace process—those who rejected both arguments were likely to be more conciliatory, those who accepted both were least likely. But most Israelis believe in some aspects of the People Apart Syndrome.

Two shortcomings of the book stand out. Disappointing is the decision to exclude the Israeli Arabs from the survey. Perhaps they "do not take part in the security debate," as Arian observes to justify their exclusion from his study, because they are never asked. The result is that nearly 20% of the population is invisible from public opinion but quite capable of shaping the outcome of Israeli elections and the peace process. Second, the book conveys such a mass of information through plentiful tables, graphs, and charts that it is easy for the reader to drown in the statistics. It seems that Arian is more concerned with presenting the immense data he has collected over the years than with trying to sew the pieces together into a guiding account.

The book begs for a postscript regarding the events that have occurred since it went to press: the assassination of Rabin, the election of Netanyahu, the status of the peace process. I look forward to this edition. Until then, I highly recommend this book to students of political behavior and comparative politics as well as to those who want to gain a better understanding of how changing views of the Israeli public affect the peace process.

The Dynamics of Foreign Policy Analysis: The Carter Administration and the Neutron Bomb. By Vincent A. Auger. Lanham, MD: Rowman and Littlefield, 1996. 160p. $52.50 cloth, $21.95 paper.

Richard J. Payne, *Illinois State University*

The end of the Cold War is challenging many of the assumptions on which American foreign policy is made as well as theories explaining the policymaking process. Vincent A. Auger finds the dominant conceptual frameworks, single-factor analyses and integrative analyses, inadequate. Single-factor analysis concentrates on the influence exerted by a single individual or institution on foreign policy decisions. Integrative analysis, by contrast, attempts to demonstrate how patterns of relationships among individual components of the policy process influence policy outcomes. The author's principal objective is to offer a supplementary perspective to prevailing modes of conceptualizing the foreign policy process.

Auger believes that the general preoccupation with the methodological elegance of spare theoretical models and parsimony pursued for the purpose of theoretical rigor often prevent foreign policy analysts from presenting a more accurate analysis of how particular foreign policies were formulated. He argues that there are three major weaknesses in most models of foreign policy analysis. First, these models provide an essentially static analysis of the dynamic and fluid process of policymaking. Second, they do not adequately portray the sequential aspects of policy choice, largely because they tend to be ahistorical. Third, most categories of analysis concentrate on domestic actors and influences and generally disregard or downplay how international factors, such as negotiations with allies and adversaries, domestic realities within countries that are affected by American foreign policies, and general international systemic constraints affect policy formulation in the United States.

Auger calls for a more inclusive, comprehensive, and dynamic framework that is more sensitive to the dynamic nature of the policy process, the twists and turns, values of policymakers, and the political trade-offs inherent in the formulation of foreign policy. This approach underscores the need for foreign policy analysts to cross disciplinary boundaries. He believes that this more complex and nuanced perspective will reward those concerned with foreign policy with a better understanding of the past as well as the present. Furthermore, it provides a more accurate gauge for analyzing future policy choices.

To accomplish his objective of offering a supplementary perspective to prevailing modes of conceptualizing the foreign policy process, Auger presents a carefully researched and detailed analysis of the Carter administration's handling of decisions on the production of the neutron bomb. Instead of limiting his analysis to bureaucratic politics, executive-legislative relations, and alliance politics, Auger examines all the major participants in what he calls "the policy making odyssey of the neutron bomb." These include the president and his senior advisors, various executive branch bureaucracies, the Congress, the North Atlantic Treaty Organization (NATO) allies (particularly West Germany), and the Soviet Union. While such a broad focus lacks the neatness of spare theoretical models, it clearly shows the complex nature of decision making and how presidents are constrained by decisions made by previous administrations.

Auger discusses the historical context of Carter's decision on the neutron bomb. President Dwight D. Eisenhower, Secretary of State John Foster Dulles and the Chairman of the Joint Chiefs of Staff Admiral Arthur W. Radford believed that nuclear weapons would reduce military costs and simultaneously protect U.S. global interest. Eisenhower's strategy, called the New Look, was challenged by the Soviet Union's production of nuclear weapons and its ability to launch intercontinental ballistic missiles (ICBMs). No longer could the United States and its NATO allies use nuclear weapons without risking Soviet retaliation. Furthermore, conflicts between the superpowers in Europe threatened the nuclear devastation of the United States. This new reality strongly influenced President John F. Kennedy and his secretary of defense, Robert McNamara, to regard nuclear weapons very differently from the view of the Eisenhower administration. The distinction between nuclear weapons and conventional forces was sharpened, and the goal of American foreign policy was changed from using nuclear weapons in Europe to maintaining a credible deterrent in Western Europe and diminishing the likelihood of a nuclear war between the United States and the Soviet Union. While the Nixon and Ford administrations adopted policies consistent with those of the Kennedy-Johnson administrations, the former believed that nuclear weapons could be used to prevent escalation to a full-scale nuclear war between the superpowers. Limited nuclear options (LNOs) were seen as effectively lessening escalatory pressures and simultaneously bolstering the credibility of the U.S. deterrent. Budget cuts in the aftermath of the Vietnam War strengthened this perception.

President Carter, who was confronted with strong U.S. and NATO support for several theater nuclear weapons, had publicly committed his administration to diminishing U.S. reliance on nuclear weaponry. Indeed, his long-range goal was to "eliminate all nuclear weapons from the face of the earth." Carter's own values and views on nuclear weapons played a crucial role in the neutron bomb decision. Fearing that endorsing production of the neutron bomb would undermine his credibility, Carter essentially allowed the Pentagon to cultivate public and institutional support for the neutron bomb. Moreover, few officials in the Carter administration were knowledgeable about this weapon. Congressional opposition to Carter's defense and foreign policies in general and disagreements between National Security Council officials and the president's political advisors on the neutron bomb only complicated the administration's decision-making process.

One of the most important contributions of this book is the author's discussion of how the Carter administration involved the European allies in the decision on the neutron bomb to obtain a negative decision on its production. Although Auger correctly points out that Carter's decision to ask the NATO allies to support deployment of the neutron bomb before it was built was unprecedented, it could also be argued that the president's consultation with allies who were directly affected by the decision was a healthy development in alliance relations. Contrary to Auger's view that expecting the allies to make a quick decision on such a controversial issue "was truly optimistic," giving the Europeans a decisive role was consistent with Carter's position on nuclear weapons. In light of the dominant views within the Carter administration, Auger clearly overstates the point when he claims that administration officials were dismayed by the reaction of the allies to the neutron bomb's notoriety.

The influence of domestic politics both in the United States and Europe on American foreign policies is clearly documented. The political left in West Germany strongly opposed deployment of the neutron bomb. Many German military officers feared that placing this weapon in the heart of Europe would exacerbate problems with the Soviets, threaten detente, and trigger a nuclear war in Germany if conflict erupted. Germany's central foreign policy objective of Ostpolitik would certainly be undermined by actions the Soviets deemed provocative. In fact, as the author points out, the Soviets helped to influence American policy on the neutron bomb by launching a campaign in Western Europe against it. Apart from damaging West German relations with East Germany, the neutron bomb debate put the Soviet Union in a position to criticize Carter on human rights and nuclear proliferation. The Soviets stressed that the neutron bomb threatened to accelerate the nuclear arms race and was inconsistent with Carter's crusade for human rights. Domestic opposition to nuclear weapons in the United States strengthened opposition from abroad. Furthermore, the neutron bomb decision had to be placed within the broader context of U.S.–Soviet relations.

But the author's willingness to embrace general criticisms of Carter's decision-making style effectively undermines many of his excellent insights on the complex nature of foreign policy formulation and implementation. Auger fails to show how what he regards as Carter's indecisiveness created the debacle which occurred in relation to production of the neutron bomb. Given the Soviets' strong opposition, threats to the more significant Strategic Arms Limitation Talks, lack of support from the European allies on whose territories the neutron bomb would be deployed, and the president's own values, was Carter's decision not to produce the neutron bomb a debacle?

While some foreign policy analysts will undoubtedly find Auger's approach to be too comprehensive, the author clearly demonstrates that only by considering many factors can Carter's decision on the neutron bomb be accurately understood. *The Dynamics of Foreign Policy Analysis* points to the need to recognize the usefulness of a comprehensive analysis of foreign policies. Although the book's focus on the narrow issue of the neutron bomb will limit its appeal to a general audience, students and practitioners of foreign policy are likely to find this case study to be extremely useful. Even though this analysis of foreign policy is not as innovative as the author claims, it is nonetheless an important contribution to the growing body of literature on how domestic and international factors interact to influence American foreign policies.

Imperial Encounters. By Roxanne Lynn Doty. Minneapolis: University of Minnesota Press, 1996. 213p. $69.50 cloth, $27.75 paper.

Craig N. Murphy, *Wellesley College*

Imperial Encounters is an important and in some ways pathbreaking study of the international political economy. Doty's goal—to explore the practices of representation by the North of the South—has long been pursued by literary critics, historians, anthropologists, and others, but only recently has it become an aim of some other American political scientists, none of whom have developed as wideranging an argument as the one presented here.

Doty defines imperial encounters as asymmetric interactions in which "one entity has been able to construct 'realities' that were taken seriously and acted upon by the other entity" (p. 3). Her specific cases include U.S. relations with the Philippines, British relations with Kenya, the democracy and human rights issues that have linked some states and citizen groups in the North to some Third World states since the 1960s, and the concurrent North American academic discourse on North-South relations. Doty uses her cases to construct a genealogy that explains how the modern bifurcated world identities work to limit the political practices of those on both sides of the divide. She searches for the ways in which the North-South discourse fixes meanings through naturalization (the presumption that some things are "naturally" true), through systems of classification and of surveillance that organize knowledge about the differences along which the identities are defined, and through the negation of information that does not fit within the emergent scheme.

Close readings of legislative debates, reports of colonial administrations, and academic treatises on the developing nations support Doty's central claims. (1) A shared, unexamined white racism helped American advocates of the annexation of the Philippines prevail over their fellow citizens who opposed it. (2) A rhetorically masked need for the forced labor of African natives structured British policy in East Africa. (3) In both cases, the successful construction of the colonized natives as immature—children who have yet to become autonomous and responsible—served to justify the colonizer and former colonizer's violent counterinsurgency policies of the 1950s.

The analogy between children and Third World nations continues at the core of the North's programs to promote development, democracy, and human rights in the South. Unlike the Marshall Plan, which was administered by Europeans, programs for aiding the Third World continue to be based on the assumption that the West must define goals and closely monitor the progress of peoples who have not yet attained the age of reason and discretion (p. 134).

Doty argues that the same constructed difference between the mature, responsible North and the immature South remains an unexamined premise of even some of the most insightful recent international relations analysis of North-South relations, such as that of Robert H. Jackson and Stephen D. Krasner. Even though some may not agree with Doty's close reading of these authors, she argues her case well and in doing so concludes a strong historical and empirical demonstration of the insights that can be brought to international political economy by her "dissident," "postmodernist," interpretive methods.

Nonetheless, many of Doty's separate conclusions have also been reached by anthropologists, historians, geographers, or political scientists employing interpretive methods influenced more by Marx or Weber than by Derrida and Foucault. Doty is careful to acknowledge such convergent conclusions, but the strictures of a short book do not allow her to explore differences in the overall historical explanation of North-South relations offered by her and by other interpretive scholars.

These differences may be especially salient when it comes to assessing prospects for transforming the current political inequality between North and South. Doty reminds us that Gramsci always treated the historical analysis of the contradictory layers of our consciousness as the first step toward political emancipation (p. 163). It is equally important to remember that Gramsci treated it as only the first step, to be followed by a concrete analysis of the existing system of production, the rational interests and aspirations that the system can sustain, and the prospects for progressive political alliances among the political actors pursuing those plausible goals. It would have been helpful if *Imperial Encounters* had suggested more about what the most valuable next steps in a "dissident" research program might be.

The text also could have been stronger if Doty had worked with a wider range of primary materials. Scholars in other disciplines who have used the same methods to explore North-South relations have gone beyond Doty's sole reliance on written texts by employing music, literature, sculpture, paintings, cartoons, architectural drawings, and maps to illustrate how the powerful have shaped the naturalized, modern vision of the South. Such alternative "texts" can especially vividly demonstrate the techniques by which Third World subjects have been negated, for example, through the importation of visual tropes of the exotic from "artistic" representations of non-European peoples into generations of putatively "scientific" drawings or through the erasure of non-European place names and other knowledge from "scientific" maps.

These are minor points. *Imperial Encounters* remains significant for all students of international political economy, especially those, like myself, who begin with skepticism about the usefulness of postmodernist analysis for the study of the inexorably "material" issues of global inequality. Doty's book clearly demonstrates the value of such analysis.

Nuclear Weapons and Arms Control in the Middle East. By Shai Feldman. Cambridge, MA, and London: The MIT Press, 1997. 336p. $18.00.

Amit Gupta, *Stonehill College*

Much of the debate about nuclear weapons and arms control is centered in the western world. Among new or threshold nuclear states only in India and Israel has significant thinking gone into these issues. Shai Feldman's book is one more competent Israeli effort on the subject. The author discusses the prospects for nuclear arms control in the Middle East by examining the major issues involved in this process: the status of each country's nuclear efforts; the policies and attitudes of both the Arab states and Israel toward arms control; and how the proposed policies of each side heighten the security dilemma of the other. He concludes by suggesting three interim measures for regional arms control.

With the forced termination of Iraq's nuclear program, Israel has a nuclear monopoly in the region, and its security concerns would have to serve as the basis for any future arms control discussions. Feldman argues that Israel's policy is based on several assumptions. First, only after a genuine peace is achieved in the region can successful arms control be implemented. Second, Israel would prefer a regional regime like Egyptian President Husni Mubarak's call for a Weapons of Mass Destruction Free Zone (WMDFZ) rather than a global regime like the Nuclear Non-Proliferation Treaty (NPT). Israel's unwillingness to enter the global regime stems from the NPT's lack of intrusive verification to check if safeguards are being violated and because it allows a country to develop a nuclear fuel cycle, thus making it possible to move quickly to a full-fledged nuclear program. Third, Israel would want a regional regime in which all the states of the region are included.

In contrast, and reflecting the different political agendas of particular states, there is no single Arab policy toward arms control. Instead, there are three. Egypt would like Israel to sign the NPT or adhere to a WMDFZ regardless of the lack of progress on the peace front. Smaller states like Jordan and those of the Gulf region are less concerned about the nuclear threat and would expect nuclear arms control to emerge from a more broad peace process. Finally, there are those states which have still not recognized Israel and, therefore, would not be part of any future arms control negotiations—Iran, Iraq, and Libya.

These conflicting views on arms control are further complicated by the fact that nuclear weaponry has to be linked to control of other types of weapons as well. Given Israel's quantitative inferiority in conventional arms it would like to see reductions in these types of weapons as part of a regional package. It would also like to see unconventional weapons, such as biological and chemical weaponry and ballistic missiles, brought into a future regime. At the same time, the Arab states see nuclear arms reduction as a priority, particularly in view of the Vaanunu revelations of 1986 that Israel had an advanced nuclear force capable of fulfilling a range of missiles. In fact, Feldman suggests that Arab security analysts believed that Israel had a limited nuclear arsenal which was to be used to provide a deterrent against Arab attacks on Israeli territory. The Vaanunu revelations were disquieting because they led to the assumption that Israel had moved from a policy of nuclear deterrence to one of having a nuclear warfighting capability (p. 128). In such circumstances, giving up a chemical or biological arsenal without significant cuts or even the total elimination of Israel's nuclear stockpile would be suicidal.

In light of the constraints on both sides, Feldman suggests three interim measures for arms control which would begin to address Arab concerns in the nuclear realm without damaging Israeli deterrence. The first is that Israel enter into informal discussions with Egypt and Jordan, states with whom its political relationship has undergone a significant transformation, on the implications of nuclear proliferation and deterrence in the Middle East. The second is a wider discussion with other regional states on the cut-off of fissile material production, thereby restricting the development of weapons-grade material. The third is to use arms control and regional security talks as a forum for Arab-Israeli discussions regarding the prospects and conditions of the denuclearization of the Middle East.

Feldman's book is balanced in its description of the security dilemma of both sides in the Arab-Israeli conflict. It would serve both as a good textbook on proliferation and arms control in the non-Western world as well as a ready reference on the Middle East for the more advanced scholar. The book does leave some issues hanging. In the early discussion of the dangers of proliferation Feldman discounts the crazy Third World leaders who are uncivilized enough to use nuclear weapons. As he points out, civilized nations have just as bad a track record—Germany was civilized before and after World War II but still produced Hitler (p. 23). But in his discussion of Israeli fears of a potential Arab nuclear force, he once again brings out the crazy Arab argument and the belief that somehow Arabs place less value on human life.

He also accepts the Israeli argument that for peace in the Middle East only Israel can have nuclear weapons. Their possession by any other state would be destabilizing, a policy approach which can only raise concerns in Arab capitals, even more so because of the conflicts among the Arab states themselves. Feldman's suggestions for interim arms control are modest, but they reflect the complex threat environment of the Middle East. The interim measures also indirectly bring out the complexity of multilateral negotiations and therefore can serve as a lesson to other regions of the world attempting similar arms control efforts.

Finally, there is a trendy move among analysts (and Feldman at times seems to fall in this category) to suggest that the real risk of proliferation is that because the United States and the Soviet Union made a series of mistakes in their nuclear competition, newly nuclearized states are likely to make the same mistakes. Feldman initially provides evidence to refute this idea—Indian and Pakistani attempts to make the accidental use of such weapons difficult (p. 25)—but does not follow it up in his discussion of the Middle East. Overall, this book is a good addition to the burgeoning literature on nuclear and arms control issues in the non-Western world.

Dirty Tricks or Trump Cards: U.S. Covert Action and Counterintelligence. By Roy Godson. Washington, DC: Brassey's, 1995. 337p. $24.95.

Secret Agencies: U.S. Intelligence in a Hostile World. By Loch K. Johnson. New Haven, CT: Yale University Press, 1996. 262p. $30.00.

David M. Barrett, *Villanova University*

Most political scientists probably understand something of the importance, for better or worse, of the Central Intelligence Agency (CIA) and other secretive governmental agencies in the past half-century of U.S. political history. The tremendous secrecy surrounding these organizations, however, has kept most scholars of U.S. government and politics from paying much attention to intelligence agencies. The government claims that it is necessary to shield not just current and recent intelligence activities from public (and scholarly view) but to do the same regarding many such matters from even the early decades of the Cold War. Those few scholars who have chosen, nonetheless, to examine the CIA and others have taken on a frustrating, if sometimes fascinating, specialty.

From the 1950s through the 1970s, an eminent scholar of this topic was Harry Howe Ransom, who carefully and dispassionately sorted through the available evidence and information and offered a series of books and articles providing intelligent, critical, but fair-minded analyses of the U.S. government's secret agencies. Among his chief interests was the tension between secrecy and democracy, especially the extent to which elected officials in the executive and legislative branches had knowledge of, and gave direction to, intelligence activities. Ransom has continued to give encouragement to successor scholars who have explored these topics in the late years of the Cold War and after.

Most prominently among political scientists, Loch Johnson has continued the scholarly and common-sensical tradition of reading everything available on the topic, interviewing scores of current and former intelligence practitioners and overseers, and then providing engaging and thoughtful analyses of the intelligence activities carried out by the U.S. government. One task Johnson and others have faced is becoming knowledgeable about the large number of intelligence agencies. Most citizens and scholars are aware of the CIA, but many have never even heard of the National Security Agency (NSA), which has more personnel and a bigger budget than the CIA. Nor are they familiar (for example) with the National Reconnaissance Office (NRO), the Defense Intelligence Agency (DIA), or the Bureau of Intelligence and Research (INR). These and other agencies are supposed to work together under the coordination of the Director of Central Intelligence (the DCI, who also heads the CIA) so that (1) the president and others are aware of threats to U.S. national security, (2) secret operations can be carried out, as deemed necessary by the president and others, to shape events and conditions in various countries, and (3) efforts by other countries to spy on the United States can be effectively countered. The mission is often confounding for DCIs, in part because of the tremendous amount of bureaucratic politics that occurs in the intelligence establishment. For scholars, keeping up with so many agencies shrouded in such secrecy has been maddeningly difficult. Fortunately, the government has begun to release some previously classified documents, though at far too slow a pace to suit scholars.

In *Secret Agencies*, Johnson has done an admirable job of description and both empirical and normative analysis. He writes effectively for an audience that could and should include both specialists and beginners. Among other things, Johnson wrestles with varying definitions of intelligence and the ways in which intelligence missions of agencies have evolved across the decades. Updating previous work, he confronts the messy question of the ethics of covert operations. He also gives a good overview of the increasingly important issue, given the end of the Cold War, of whether there is a need for the CIA at all. I found Johnson's analysis and/or history unpersuasive on a few points; others, no doubt, will find themselves in huge disagreement with his conclusions. He is deeply critical of certain intelligence policies carried out since the 1940s yet generally favorable toward the *raison d'être* of the intelligence establishment. His final chapter, assessing the successes and failures of the U.S. intelligence establishment in recent history, is most interesting; to do a credible job of passing judgment on such a conglomeration of activities and agencies across the better part of a half-century is no small achievement. His conclusion—that "America's secret agencies are indispensable, however many and troubling their mistakes were during the Cold War"—will anger some readers at different ends of the political spectrum. The depth of his research and the thoughtfulness of his analysis, however, present those readers with no easy target for criticism and debate. I and other scholars of intelligence can draw on Johnson's research and analysis to further our own scholarship in this difficult area.

Roy Godson is a political scientist who (like Johnson) has had substantial experience in government service. In *Dirty Tricks or Trump Cards*, Godson analyzes the history and adequacy of the U.S. government's covert action policies. This is a topic that has been much scrutinized by others, but often in a sensationalist fashion. More uniquely, Godson devotes equal attention to U.S. counterintelligence activities, a topic that has received scant treatment even from scholars of intelligence. Godson rightly argues the importance of counterintelligence and provides a substantial history.

Compared to Johnson, Godson is much more of an enthusiast for the idea of covert action. He believes, however, that the United States has not carried out covert actions very successfully in recent decades. He criticizes the now substantial influence of congressional oversight committees and the prevalence of an ethos which holds that covert actions are either improper or unfeasible. Many will disagree with parts of his history of U.S. covert action policies since World War II. For example, I do not regard U.S. involvement in the overthrow of a democratically elected government of Guatemala in 1954 as a success, though certainly the Eisenhower administration thought it was. Having said that, Godson's knowledge of political and intelligence history is impressive, indeed. Also, his treatment of the nuts and bolts of counterintelligence and covert action has great depth, more than most political scientists will require.

Godson, even more than Johnson, considers the world to be a dangerous place. The U.S. government is giving short shrift to counterintelligence and covert action capabilities, he claims, despite the billions of dollars it spends on intelligence. His suggestions for reform, written from his perspective as an intelligence specialist, are countered by his analysis in the final chapter from the perspective of a political scientist. Given the nature of our regime, the political cultures of U.S. intelligence bureaucracies, and prevailing perceptions of the international environment, it is unlikely (he writes) that we will soon alter our intelligence capabilities in ways that he recommends. Godson's analysis is well worth reading in conjunction with reports done by various committees and commissions in the executive and legislative branches during the past few years.

Postmodern War: the New Politics of Conflict. By Charles Hables Gray. New York: Guilford Press, 1997. 314p. $23.95.

Francis A. Beer, *University of Colorado at Boulder*

Postmodern War is a welcome addition to a growing body of literature that explores the changing character of modern war. This work describes the future of military activity as it is driven by such contemporary technologies as telecommunication, computers, video, and robotics. Physical dimensions of war are the most obvious loci of change, but there are also more subtle, subjective transformations in the nature and meaning of war.

Postmodern war emerges from prior formulations, such as antiwar, communications war, computer war, culture war, cyberwar, electronic war, high modern war, high-tech war, hypermodern war, information war, low-intensity war, neocortical war, netwar, permanent war, postmodern war, pure war, scientific war, space war, technowar, technological war, television war, video war, and virtual war. It draws from interpreters of military doctrine like Arquilla, Ronfeldt, and Van Creveld, futurists like Toffler, and postmodern theorists of war like Baudrillard, De Leuze and Guattari, Foucault, Virilio, and Lotringer.

The central symbol of postmodern war, in Gray's work, is the cyborg soldier, a "cybernetic organism—a human integrated with a machine. That the cyborg is coming true on the battlefield and that he (!) is a central metaphor of discourse about future war is an important theme of this book" (p. 9). Gray traces the emergence and evolution of the cyborg through historical and contemporary battlefields. The destructive achievements of technology at Hamburg, Dresden, Tokyo, Hiroshima, and Nagasaki; Warsaw, Rotterdam, and Coventry; Nanking; Treblinka, Auschwitz, and Dachau foreshadowed later developments (p. 22). Vietnam and the Gulf War showed the scope and limits of evolving military technoscience. Drug wars, terrorist wars, and space wars are contemporary growth areas, and there is a rich description of U.S. military plans for the millennium and beyond.

The book considers multiple dimensions of war, including its identities as policy by other means, communication, entertainment, science fiction, and eschatology. It is a good review of the latest military technology, with lots of material on the most recent and smartest war toys. It goes beyond the bounds of a narrow military primer in its discussion of the broad role of science in modern warfare and in its consideration of rhetorical and psychological warfare. Political scientists interested in politics and science, politics and the military, peace and war, and virtual and postmodern politics will find it interesting and useful.

The State, War, and the State of War. By K. J. Holsti. Cambridge: Cambridge University Press, 1996. 254p. $59.95 cloth, $18.95 paper.

Russell J. Leng, *Middlebury College*

Since World War II, the traditional view of the relationship between the state and war among American students of international politics has been one version or another of political realism. States are treated as unitary actors who compete with one another for power and influence in an anarchic global system. Interstate war is seen as a natural consequence of the unrestrained competition and insecurity of states. The realist perspective, however, has never been without its challengers. K. J. Holsti's provocative new book is the most recent entry in a long-line of revisionist views of the relationship between states and war.

In the late 1950s John Herz's *International Politics in the Atomic Age* (1959) predicted the imminent demise of the territorial state, or at least its *raison d'etre*. Herz maintained that the major historical arguments for organizing polities into territorial states—security and economic prosperity—no longer fit the realities of an age of atomic weapons and the economic integration he saw beginning in Europe. But the state survived, and Herz himself ultimately had second thoughts. Twenty years later, Keohane and Nye's *Power and Interdependence* (1977) challenged the realist image of a global system dominated by states competing for power and security. They argued that new networks of communication had weakened the unitary character of the state and that economic issues often bumped security off the top of the foreign policy agenda. For Keohane and Nye it was not nuclear weapons and economic integration that had rendered the realist image of the state obsolete, but the technological revolution in communications and the global marketplace. By the 1980s John Mueller was arguing, in *Retreat from Doomsday* (1989), that it was not the state but *war* that had become obsolete, at least among the major powers. Observing the long peace among the major powers since World War II, Mueller drew the conclusion that maturity borne of brutal experience led those states to a sober recognition that war was morally revolting and economically wasteful. With the end of the Cold War and the collapse of the Soviet empire, one of the most striking features of the current decade has been the transition of formerly authoritarian regimes to democracies. Writers in the 1990s have seized on a statistical fact, initially noted by Small and Singer ("The War-Proneness of Democratic Regimes," *Jerusalem Journal of International Relations* 1 (1976):1–50, 69), that democracies rarely, if ever, have fought wars with one another. Mueller's "long peace" has become the "democratic peace." The contemporary argument is that it is not a more mature understanding of war's costs and consequences but shared political cultures of compromise and decision-making systems with opposition parties that have promoted peace among democratic states.

Holsti, who has written extensively and well on international politics for more than thirty years, offers another take on the contemporary state-war relationship. War has not declined in our time, he argues; its locus has changed. We live in an age of internal wars; wars within states, as opposed to wars between states, now present the greatest threat to international stability. This fact, he asserts, means that most of our traditional thinking on how to deal with the problem of war is irrelevant. Holsti refers to these internal conflicts as "wars of the third kind" to distinguish them from the limited interstate wars of the eighteenth century and the total wars of the first half of this century. Wars of the third kind include wars of national liberation (Algeria), unification (Vietnam), and secession (Rwanda or Bosnia). Their attributes are all too familiar to us. They usually are long wars of attrition, fought with guerrilla tactics and terrorism. They obliterate the distinction between combatants and noncombatants, devastate communities, produce high casualty rates and waves of refugees, and exacerbate already seemingly irreconcilable differences. They are not fought over territory per se but over issues of statehood and relations among communities within states. Holsti believes that such wars will continue to persist in weak states, that most recently formed states are weak, and that they are weak largely because they have been formed out of such wars.

Holsti's cure for the problem is to construct internally strong states. He equates a state's internal strength with its capacity to obtain legitimacy. Holsti's state-level prescription for the prevention of intrastate conflict, in fact, parallels a traditional system-level prescription for the prevention of interstate war which was offered three decades ago by the realist Henry Kissinger. Kissinger argued, in *A World Restored* (1964), that the key to interstate peace lay in a shared sense by the major powers that the existing order was legitimate. Holsti makes essentially the same argument at the state level: just substitute the state for the system and constituent communities for major powers. That, of course, does not make it a bad argument. Kissinger's notion of legitimacy was based on the degree to which the most powerful members of the system accepted the principles on which the order of the system was based. Eighteenth-century Europe found itself in a general war when one of its most powerful members, Napoleonic France, did not accept the legitimizing principle of an order based on dynastic state regimes. Holsti makes a similar argument at the state level when he emphasizes the importance of "the principle on which the 'right to rule' is based." He is well aware, however, that in the contemporary world, debates over principles are often motivated by emotional attachments and personal grievances. Holsti gives less weight to instrumental measures of legitimacy, such as the regime's ability to achieve economic prosperity or to deliver services. Wars of the third kind are not fought over the delivery of services.

How can we create a world of "strong" states? Holsti, to his credit, does not view democratization as a panacea, particularly in societies with strongly divided communities. He also questions whether the Western model of the nation-state is appropriate to all societies. But Holsti's sense of what is not likely to work is better than his blueprint for obtaining what will. In the end, he can offer only the thread-bare faith of an internationalist academic. We must work through the United Nations as the only truly international body, but first we must reform the U.N. to enable it to deal properly with the new problems of peace making and state building. Given its poor performance record to date, and its current financial and political woes, banking on the U.N. to meet challenges of the scope described by Holsti seems utopian. Holsti tends to view the U.N.'s ineffectiveness in peace making and state building as a function of a weak understanding of the problems, and he suggests that we devote more attention to studying what makes strong polities. The problem lies deeper than that. Holsti's prescription, in fact, ignores one of the less attractive attributes of strong states: They are possessive of their independence and distrustful of providing executive power to intergovernmental organizations. Or as a realist might put it, they are selfish entities that are possessive of their power in a world that they continue to view as essentially competitive. That is the deeper explanation for the weak track record of the United Nations and its current financial woes. But it would be unfair to expect the author to provide a practical solution to a problem of this magnitude. Holsti has provided a sufficient service by clearly identifying the nature of the problem.

The State, War, and the State of War is an intelligent and provocative discussion of the most serious international problem of our time. The book is also a pleasure to read. It is written in a clear and graceful style; the arguments are supplemented by relevant real-world examples. It would be an excellent choice as a supplementary text for upper-level undergraduate or graduate courses. There is also a useful appendix, which lists and categorizes those disputes from 1945 to 1995 that qualify as wars under Holsti's criteria.

Why Syria Goes to War: Thirty Years of Confrontation. By Fred H. Lawson. Ithaca, NY: Cornell University Press, 1996. 296p. $35.00.

Allison Astorino-Courtois, *Texas A&M University*

This book has an engaging title indeed. For students of Middle East politics, and the Arab-Israeli conflict in particular, it promises exploration of an enduring and often debated question. Lawson frames this exploration in terms of the relationship between "the conflicts that shape the domestic political economy and the strategies that governments adopt during confrontations with external adversaries" (p. 1). The book's six cases thus follow political-economic forces at work inside Syria to explain Syrian participation in the 1948 and 1967 Arab-Israeli wars, the 1970 Jordanian Civil War, 1976 military intervention in Lebanon, and two cases in which "open warfare" was avoided: a bellicose period with Iraq in 1982 and with Turkey in 1994.

Notions borrowed from Marxian analysis are employed to illuminate what is essentially a diversionary war explanation of Syrian conflict behavior, extended by reference to political-economic motives. Lawson argues that diversionary tactics can take the form of either participation in external conflict or a policy of accommodation in interstate crises. When the regime faces a significant challenge, diversionary tactics will take the form of preparing the country for and participating in militarized conflict. Specifically, belligerent Syrian foreign policy can be explained by three factors: "accumulation crises," high levels of domestic political conflict, and "contradictory regime responses" (p. 157). To wit, labor and public and private capital forces—components of the dominant social coalition—in Syria compete with and challenge one another. The tendency of these groups to work at cross-purposes in responding to difficult economic and social issues fosters accumulation crises or, roughly, imbalances among them. Under conditions of extreme political-economic conflict, the parochial efforts of various groups to shore up their own interests, when given an external opportunity, actually impel Syria to war or military action. Alternately, when conflict within or opposition to the dominant coalition is relatively weak—but presumably great enough for some need to divert public attention—the government's diversion will take the form of an accommodative posture in an international crisis. Thus, Lawson presents a model of economically driven diversionary tactics by Syrian regimes to explain both *Why Syria Goes to War* and why it does not.

In undertaking this analysis, Lawson succeeds in laying out a well-researched and exceedingly detailed chronicle of the political-economic forces operating in Syria at various periods from the 1960s until today. He has amassed a wealth of domestic political and economic information, much of which must have been very difficult to locate and verify. If only in this regard, the book should be a welcome addition to the scholarship on Syrian society and politics. That is, as a book on Syria, this volume clearly can contribute to a relatively narrow literature. Regarding the more ambitious objective, however, which is suggested both in the presentation and title of the book—to provide an explanation of Syrian conflict behavior—it has some serious shortcomings. By and large, these have to do with an incomplete theoretical framework, including a series of important but unstated assumptions.

First, in defending his approach, Lawson notes that existing accounts largely explain Syrian foreign policy "on the basis of leaders' idiosyncrasies, the sectarian nature of the regime, or the strategic circumstances in which Damascus found itself." These approaches present biased and incomplete explanations, he argues, because they ignore "political economic factors as primary determinants of the policies that governments adopt in crisis situations" (p. 169). Having completed this review, however, the book unfortunately provides no evidence that the political-economic cleavages uncovered in each of the cases examined were actually connected with Syrian foreign policy behavior.

In fact, Lawson appears to have presumed a priori the soundness of a diversionary explanation rather than testing the degree to which it works in the Syrian case. Moreover, the argument appears to rest on the (unexplained) presumption that diversionary tactics will *always* be used. For example, in explaining Syrian participation in the 1967 war against Israel, Lawson lays out an interesting survey of social and political-economic forces in Syria during the difficult period of Ba'thist consolidation in the early 1960s. Yet, even if the Syrian decision elite did use preparation for war with Israel to reorganize conflicting domestic groups as Lawson argues, it is unclear how the domestic interests which might explain a public mobilization effort account for a move to *use* these forces to confront the not insignificant Israeli military. Subsequent Syrian moves suggest an alternative: Why not turn Syria against the enigmatic Jordanian regime, a constant target of Ba'thist antagonism in those days, and certainly a more certain victory than a battle with Israel?

Although the connection among Syria's domestic unrest, economic ills, and decisions for military intervention may be surmised, the processes that link the penultimate to the ultimate step in Lawson's model are never discussed. True, while at some points in the analyses reasonable suppositions of the issues Syrian elites *may* have considered in making a decision for military action are provided, there is meager evidence that any element of the Syrian elite actually *did* act to manipulate international affairs in order to stem domestic difficulties. Instead, lacking discussion of these important linkages if only theoretically, Lawson presents an account of simultaneous domestic economic and interstate events from which a causal relationship between the internal factors and external behavior appears to have been inferred.

Relatedly, the (causal) flow of the relationship between domestic political-economic challenges and Syrian conflict participation seems to be presumed as unidirectional. Could not external threats and violence serve to dissolve domestic consensus, or even domestic indifference/complacency with the regime, rather than the other way around? A fair amount has been written, for example, that postulates a recursive relationship between Syria's choice to participate in the 1970 Jordanian Civil War and challenges within, and later to, the regime (or at least the dominant Jadidist faction). The point is that the causal direction is not discussed but left as another important yet unstated supposition.

At the end of the book Lawson addresses the implications of his analysis. Seemingly without qualification, he concludes that the probable result of significant intraregime conflict will be external war: "Confronted with deepening splits inside the dominant coalition, which both strengthen forces challenging the regime and make it more difficult to resolve domestic accumulation crises, *almost any state* can be expected to risk war rather than accept the certainty of internal collapse" (p. 181). Here again, it is unclear whether Lawson is suggesting that the same factors which explain conflict behavior in Syria—a small, less-developed, and closed regime—should also explain those in large and/or liberal states. Would the model be appropriate for analysis of the former Soviet Union? Eastern Europe? Are we in the United States to expect an attack anytime soon from Canada?

Arguing that the decision calculuses of governments facing domestic economic and political challenges will be different from those that do not is not necessarily a novel explanation of interstate conflict. That said, however, readers may find the value of *Why Syria Goes to War* in its articulation of an alternate—if presently loose—framework for analysis of Syrian conflict behavior. For others, it may serve to rekindle the stalled debate over diversionary war explanations of interstate conflict, a literature that to date remains inconclusive.

Does Conquest Pay? The Exploitation of Occupied Industrial Societies. By Peter Liberman. Princeton: Princeton University Press, 1996. 272p. $35.00.

Charles A. Kupchan, *Georgetown University*

This book is a sobering antidote to the current wave of literature arguing that predatory war is unprofitable and obsolete. Contemporary scholars point to the peaceful end of the Cold War, the spread of democracy, the information revolution, and the broadening and deepening of the global economy to make the case that a new and more stable era of international politics is opening. Liberman offers an important counterweight to this optimism, warning that conquest still pays in the industrial era and that predatory states still have strong incentives to go to war.

Liberman makes an important contribution on three counts. First, he provides compelling evidence that victorious powers can increase their material strength by extracting resources (wealth, goods, and manpower) from the vanquished. Liberman mines the available evidence from a representative set of twentieth-century case studies to show that aggression is at times very profitable; aggressor states can extract from occupied states far greater resources than they expend on occupation and repression. Liberman's careful documentation of profitable war among industrialized societies makes his claim definitive.

Second, Liberman challenges the common assertion that industrialization and modernization make profitable conquest more difficult. Modernization is associated with nationalism, urbanization, and more efficient social organization, which many scholars contend render occupation and repression more costly and painful (e.g., Richard Rosecrance, *The Rise of the Trading State*, 1986). Liberman convincingly argues that modernization facilitates rather than impedes coercion and allows occupying powers to more effectively commandeer resources and manpower. The most successful cases of profitable conquest—Nazi Germany in Western Europe and the Soviet Union in East Germany—are precisely those in which the aggressor vanquishes more advanced, industrialized countries.

Third, Liberman documents the capacity of ruthlessness to overwhelm resistance and induce compliance. The most effective aggressor states are those that terrorize occupied populations. Ruthlessness intimidates and therefore obviates the need for costly occupation forces and operations. Fear also induces subjugated populations to continue performing their jobs despite resentment toward the occupying power. The Belgian and French occupation of the Ruhr-Rhineland in 1923–24 fell short of expectations precisely because the Belgians and French faced resistance and did not react with the severity necessary to induce compliance. Needless to say, Nazi rule of Western Europe did not suffer from the same shortcoming and was accordingly more profitable.

Liberman thus demonstrates convincingly that conquest can pay and elaborates on the conditions under which it does so. At the same time, however, his case studies also suggest that profitable aggression is more the exception than the rule. Indeed, although the case studies by no means cast doubt on the book's main finding, they do make clear that Liberman's conclusions need to be qualified in significant ways and that the international system may well not provide the strong incentives for conquest that he suggests.

The cases in which conquest was highly profitable were in fact quite few in number. The Nazis did best with smaller countries in Central and Northern Europe; their ability to extract more resources than they expended declined as the scope of their ambition broadened. Japan was able to profit from aggression as long as its reach was confined to Manchuria and northern China. As it moved more deeply into China, the material costs of the war far outweighed the benefits. In similar fashion, the Soviets were able to extract considerable resources from East Germany, but other areas of Soviet occupation were far less profitable. In addition, the profitability of even the most lucrative occupations was short-lived; returns tended to decline over time (p. 150).

Profitable conquest also depended on a level of ruthlessness that is fortunately quite rare, even among aggressor states. The Nazis were able to repress occupied populations at relatively low cost but only by terrifying them into submission through murder and unspeakable brutality. Conquerors unwilling to resort to similar levels of ruthlessness were less profitable. Moreover, the same processes of modernization that enable states to conquer their industrialized neighbors tend to moderate policies and make the ruthlessness necessary for profitable exploitation less likely.

The formation of balancing coalitions poses another constraint on profitable conquest. Status quo powers ally against predatory states precisely because they realize that conquest can pay and that unchecked aggressors may ultimately acquire preponderant power. Iraq's 1991 invasion of Kuwait had the potential to pay handsomely. But it ultimately did not do so because an American-led coalition, fearful of Iraq's regional hegemony, responded by repelling the invasion force and pummeling Iraq's military and war-making infrastructure. In similar fashion, the resources that both Germany and Japan extracted from their victims during World War II were more than offset by the eventual costs of going to war against the allied powers.

The German and Japanese cases do raise an interesting counterfactual. Had Germany restricted its war effort to Central and Western Europe and Japan contained its ambitions on the Asian mainland to Manchuria and northern China, both powers may well have been able to engage in profitable aggression without provoking a balancing coalition. In reality, however, aggressors are rarely able to exercise such restraint and resist the temptation to engage in further expansion. The search for additional resources as well as the temptation to try to defeat balancing coalitions while they are still gaining strength make it difficult for aggressors to restrict the scope of their ambition.

Finally, Liberman overstates the profitability of conquest by failing to distinguish more carefully between occupation and empire. Occupation entails invasion and extraction through the use of force. Empire also entails coercive extraction, though often through more subtle and therefore less costly means. By coopting peripheral countries and seeking to control them through a host of mechanisms other than unadorned coercion, imperial formations often offer benefits that invasion and occupation do not. Conquest may pay, but empire may pay even more.

Liberman acknowledges most of these objections at various points in his book. Accordingly, they do not call into question his analysis or his basic conclusion. They do, however, suggest that conquest may not be quite as profitable as Liberman suggests, providing at least a bit of hope that predatory aggression may well be past its heyday.

Sovereign Creations: Pan-Arabism and Political Order in Syria and Iraq. By Malik Mufti. Ithaca and London: Cornell University Press, 1996. 286p. $35.00.

Steven Heydemann, *Columbia University*

International relations theory has often taken for granted precisely the attributes that Middle Eastern states have found most elusive. As Malik Mufti demonstrates in this important account of the relationship between domestic politics and foreign policy behavior in Syria and Iraq, realist theories reflect notions of what states are and how they behave that bear little resemblance to Middle Eastern realities. In particular, theories of foreign policy behavior and of alliance formation that start from the assumption of states as unitary and rational actors tend to accept as given a level of stateness and a notion of state interest that have been notable for their absence in the Middle East for much of this century. If states are security maximizers, Mufti asks, then what explains the willingness of Arab ruling elites to treat state sovereignty with a stunning level of disregard, devoting tremendous energy to pan-Arab efforts explicitly intended to bring about the dissolution of individual states and their absorption into a larger entity? Moreover, what explains the increasing salience of stateness as a factor in the foreign policy behavior of Arab states in the past two decades despite the powerful legacy of pan-Arabism in inter-Arab diplomacy?

For Mufti, the answer to these questions rests in a theoretical framework that takes domestic politics seriously and that links changing patterns of foreign policy behavior to changes in the degree of state consolidation over time. Without such a framework, he argues, theoretical accounts of foreign policy behavior will not be able to explain adequately the alignment patterns among Arab states, why these patterns change, and, perhaps most important for Mufti, why the rate of change in alignment patterns slows so dramatically beginning in the 1970s.

According to the author, for example, Stephan Walt's theory of alliance formation (*The Origins of Alliances*, 1987), with its emphasis on systemic factors, neglect of domestic politics, and fixed understanding of stateness, misreads as alliance behavior a number of unity attempts by Arab elites that have little to do with balancing behavior among states. Instead, these efforts must be understood as a form of interelite diplomacy in which state sovereignty is perceived as a resource that can be negotiated away in the pursuit of regime legitimacy and stability. What is at stake in these ventures is not state security but ensuring the persistence of a ruling elite. The political interests of these elites not only are separable from some abstract notion of state interest but also may in fact require behavior—support for pan-Arab unity—that directly undermines the territorial integrity and sovereignty of "their" states. As Mufti emphasizes, "rational states do not voluntarily cede their sovereignty away . . . unity projects are best understood not as alliances between states but as elite alignments cutting across state boundaries" (p. 5).

Mufti goes on to develop a persuasive theoretical alternative to conventional neorealist accounts, treating stateness as an independent variable and explaining shifts in the level of elite support for pan-Arabism as a result of changes in the level of state consolidation. In his account, low stateness, which characterized Syria and Iraq from their independence through the start of the 1970s, encourages high elite interest in unionism. Low stateness deprives political elites of the domestic resources (largely institutional and fiscal, but also ideological and symbolic) that they need to ensure the stability and legitimacy of their regimes. Under these conditions, pan-Arabism—the notion that the Arab world constitutes a single entity that must struggle to overcome the artificial state boundaries imposed by colonial powers—represents a political resource that can confer considerable legitimacy on domestic elites striving to secure their grip on power. These circumstances incline elites to pursue one of two main forms of unionist strategy. One is what Mufti calls defensive unionism, a tactic intended to enhance the elite's legitimacy at home and strengthen its capacity to contend with domestic challengers. Alternatively, elites may engage in what he defines as expansionist unionism, a tactic designed to coopt or incorporate potential rivals either at home or abroad.

Over time, however, the ruling regimes in both Syria and Iraq benefited from growing levels of state consolidation. In separate chapters, Mufti offers summary arguments tracing the expansion of state institutional and mobilizational capacity, the growth in fiscal stability, and the successful repression of political oppositions in both Syria and Iraq. As levels of stateness increased during the 1970s, in no small measure due to the vast infusions of capital into the region after oil prices skyrocketed, elites turned to mechanisms other than unionism to achieve political stability, relying on domestic political and financial resources as the means for generating popular legitimacy and control. Despite the residual appeal of unionism, which remains an important element in Arab political discourse, it largely disappeared from the repertoire of Arab ruling elites in the period after 1970. As a result, the post-1970 foreign policy behavior of Arab states looks increasingly "state-like" and becomes more amenable to the assumptions of neorealist theory. Moreover, despite his focus on pan-Arabism, Mufti is quite sensitive to how cynical the commitment to unity was among Arab political elites, whom he depicts as captured by the popular enthusiasm for an ideology they had helped to create but endorsed for instrumental reasons, at best.

Mufti demonstrates the dynamic of these tactics in compelling detail, reviewing all major unity efforts involving Syria and Iraq. He divides his study into three main periods based on what he identifies as the three dominant systems of rule in these two states in this century—monarchy, praetorianism, and Bonapartism—but devotes most of his attention to the 1950s and 1960s as the moment of most intense pan-Arab activity. His account of these events draws not only on available secondary literature and memoirs in Arabic and English but also on declassified U.S. and British materials as well as extraordinary interview data culled from meetings with some of the leading figures in Syrian and Iraqi politics of these decades. Although Mufti is much too heavy handed in his critique of political economy accounts of these years, and at times his enthusiasm for his material leads him to offer more details than are needed, this is a very serious and largely successful effort to come to grips theoretically with a set of important problems for scholars of international relations concerning the nature of the state and the relationship between processes of state formation and shifts in foreign policy behavior.

Although Mufti views his project as an effort to integrate domestic politics more fully into neorealist claims about state behavior, his study may usefully be seen as a valuable contribution to the growing literature that seeks to challenge realist notions of the state and of state interests. It also represents a highly useful addition to our knowledge of what happened during a critical period in contemporary Arab politics. On both accounts, it is to be recommended.

Who Elected the Bankers? Surveillance and Control in the World Economy. By Louis W. Pauly. Ithaca, NY: Cornell University Press, 1997. 184p. $25.00.

Timothy J. Sinclair, *University of Warwick*

Post–Cold War international relations scholarship is a turbulent field. Much work is currently devoted to the effort to reestablish agreement about the likely center of gravity in the emerging world order. Many contributors to this literature, especially those from the liberal and critical camps, argue that the nature of the actors and the pressures on them are now quite different. New problems require innovative analyses and solutions. This welcome book asks scholars to take a different view. Yes, new problems do exist, but the core actors have not changed significantly, and new pressures, such as the reemergence of international capital mobility, must be analyzed in relation to preexisting constraints, in particular, the existence of sovereign states.

Pauly's central claim in this ironically titled book is that international relations are increasingly characterized by a clash between a new borderless economic logic, typified by capital markets, and resolutely national political demands, as citizens continue to expect their national state to take responsibility for the "broadly defined" security of the community (p. 33). He argues that states have responded to the dilemma of political legitimacy created by this contradiction in part through a collective effort in the form of such institutions as the International Monetary Fund (IMF). States have, however, not surrendered their sovereignty to markets or to the IMF because they retain veto power in the last instance. They cannot surrender their sovereign power, Pauly argues, because of the expectations of their citizens, despite the loss of considerable national policy autonomy due to the reemergence of capital mobility. The political dilemma has, Pauly suggests, served as a brake on the power of institutions like the IMF, where control over international capital mobility should logically lie. This has made the IMF a weak yet persistent institution. Why does the IMF survive despite its lack of effectiveness? Pauly argues that the multilateral economic surveillance provided by the IMF, while less than optimal, provides a context for pragmatic ongoing management, judgment, and political accommodation among states wrestling with the disjuncture between national politics and increasingly international economics.

Pauly organizes his claims and evidence in an innovative and effective manner. Rather than approach the dilemma in a functional, static way, through a schematic overview of the IMF, he leaves this task to others, and through a process of historical investigation he highlights how the legitimacy dilemma has changed and produced more or less effective management regimes over time. After a provocative introduction and masterly consideration of the main issues surrounding international capital mobility in chapter 2, Pauly devotes two chapters to the first cooperative effort at multilateral surveillance, the Economic and Financial Organization of the League of Nations, before considering the postwar development of the IMF.

This excellent book makes three contributions to the existing literature on international capital mobility and multilateral economic surveillance. Most important, it effectively argues that politics continues to matter in international relations despite global economic change, and that nothing less than thorough consideration of core political dynamics is an ongoing necessity in the study of international political economy. This is useful because much of the writing in this subfield tends to assume economic determination or is overly technical in nature. Pauly understands these latter issues (see his explication of the Mundell-Fleming model in chapter 2), but he does not get sidetracked by them. Here he is not unique, but this book is the most focused and effective statement of the approach yet produced.

Pauly's second major contribution to the literature is to take an historical approach to what others have considered in static or institutional terms. His concern with the legitimacy dilemma and the dynamic this establishes is a welcome break from incremental economic history approaches.

Third, the central aim of explicating the forces behind multilateral economic surveillance is achieved with grace and flare, "in as nontechnical a language as possible" (p. x). This is a well-written book, expertly edited, and a delight to read. Here is a major contribution in itself, given the nature of much of the literature in this field, which assumes a lot of the reader. The inviting explication makes Pauly's arguments all the more convincing and accessible to political scientists and a wider interested public not familiar with the arcane language of financial and monetary economics.

Any shortcomings in this book stem from the perspective adopted and not the analysis made within the terms of its assumptions. The first matter with which to take issue is Pauly's use of a distinction between sovereignty and policy autonomy. He argues that capital mobility does limit the degree of policy autonomy open to states. Despite this, he claims they retain the capacity, especially in times of crisis, to veto constraints on their actions and therefore must be understood to retain sovereignty. This may be true in a technical sense in reference to the canons of international law, as Pauly claims, but how meaningful is this distinction between sovereignty and policy autonomy for social scientists? Moreover, surely the wide disparities in state capacity and position in the world order make it very difficult to talk usefully about states as identical, interchangeable entities facing the same challenges? Even if the United States does retain sovereignty in Pauly's terms, does New Zealand, or the many other small and medium powers?

A further difficulty with Pauly's conception of his prime units, states, is that he sees them in unitary terms, as if states had clear, unambiguous preferences. From a certain position within states, say, the treasury or central bank, limitations on national policymaking induced by capital mobility may be seen as a positive goal, if this leads to more rapid adjustment to the exigencies of the world economy. But this might run counter to views in other parts of the state. To imply that states have established interests versus external forces like capital mobility ignores the processes through which particular interests become national interests.

Pauly's political dilemma also assumes the fixed nature of citizen preferences over time, a claim very much open to question. He observes that the Depression created certain expectations in Western societies about the appropriate role of the state, yet he does not acknowledge the possibility that globalization has shifted mass expectations in new directions. Recourse to the constraints of international competitiveness, however, seems to be a staple of national politics in most developed countries these days, and it seems reasonable to anticipate new preferences developing among citizens as a consequence.

This is an excellent contribution to the literature on international capital mobility and multilateral economic surveillance that should be read by specialists in these matters and by anyone seeking a better understanding of some of the fundamental forces shaping the post–Cold War world order. Sensitive in analysis, dynamic in its identification of crucial contradictions, thoughtful in interpretation, this book will be a benchmark for future contributions to international political economy.

Japan's Foreign Aid to Thailand and the Philippines. By David M. Potter. New York: St. Martin's Press, 1996. 206p. $45.00.

The New Multilateralism in Japan's Foreign Policy. By Dennis Yasutomo. New York: St. Martin's Press, 1995. 230p. $39.95.

David Arase, *Pomona College*

These two books are laudable attempts to update our understanding of key aspects of Japan's foreign policy, but neither wholly succeeds in offering a compelling interpretation of what they purport to study.

In the case of Potter's book, the focus is on how Japanese bilateral foreign aid policymaking includes the interests of recipient governments. Case studies of Japanese aid to Thailand and the Philippines are meant to demonstrate that the terms of Japanese aid are negotiated with (i.e., not imposed on) recipient governments. What we get, however, is only a partial and superficial view of the policymaking process that is unable to sustain other basic points made in the introduction and conclusion of this book.

The case studies are not so much exhaustive descriptions of decision making as they are an overview of Japanese-Thai negotiations over the Eastern Seaboard Project, a large-scale regional development initiative, and of Japanese-Philippine negotiations over the Multilateral Aid Initiative, essentially a stabilization program for the post-Marcos Philippines. In each case the analysis rests on an annual bilateral meeting at which donor and recipient government bureaucrats decide which projects and programs will be funded in the coming fiscal year. The fact that this routine annual negotiation takes place establishes that the Japanese bilateral aid process can accommodate recipient preferences, but this alone is a trivial finding. The larger question is how much influence the recipient has over the quantity and type of aid received from Japan. This is where the book falls short. Potter finds that "recipients get the kind of aid they request from Japan, but . . . they get it because their requests can accommodate the interests of the major actors involved" (p. xv). Because both the Philippines and Thailand learn to request the kinds of aid the Japanese are willing to fund, Potter concludes that "recipients get the kind of aid they want" (p. 153). This is flawed reasoning of course; what Potter actually describes in the case studies is how Japan teaches recipients what to ask for, and only after recipients learn the boundaries of what is acceptable are they successful in making aid requests. The description of this process is the main value of the book.

The important question of Japan's basic agenda in aid giving is not squarely confronted, and Potter's insistence that recipients have influence over Japanese aid because of an annual bilateral negotiation leaves one with the impression that Japan has no basic agenda. Japan most assuredly does, however, and among several key agenda items is the pursuit of commercial interests. In the opening chapter Potter asserts that Japanese business interests play a major role not just in influencing Japanese government priorities but in shaping priorities of recipient governments. Yet, nowhere in the body of this book does the author take up how, why, and to what effect this influence exists. Also left outside the scope of analysis is the political sphere. There are any number of examples of aid priorities being rearranged by elected political leaders who assert their authority over bureaucratic decision making. The Multilateral Aid Initiative for the Philippines is a case in point, since then-prime minister Yasuhiro Nakasone overrode the objections of the Japanese bureaucracy to greatly increase the quantity of aid to the new Cory Aquino administration. How and why high-level political intervention happens is also left unexplained. What this book tries to do is shed light on Japanese bilateral foreign aid policymaking, but it suffers from an overly narrow preoccupation with routine interactions between donor and recipient bureaucracies.

The Yasutomo book complements the Potter study in that it examines Japan's behavior in multilateral aid settings. The basic idea behind this book is to describe how Japanese activism in giving bilateral aid spread to the area of multilateral aid by the early 1990s. Yasutomo examines Japanese objectives in certain multilateral development banks, namely, the World Bank, the Asian Development Bank (ADB), and the European Bank for Reconstruction and Development (EBRD). He follows this with a study of Japanese aid policy toward Russia.

Yasutomo suggests that Japan's activism within each institution can be measured in terms of financial contributions, expansion of voting power, occupation of key management positions, and the number of Japanese professional staff in the banks. What he finds is that Japan became more active from the late 1980s and early 1990s according to these criteria, but there is variation among the cases. In the ADB, Japan sought and largely achieved a preeminent role; in the World Bank, Japan sought both increased status and a change in basic lending policy, but achieved only the former; and in the EBRD, Japan remained satisfied with what amounted to token participation. Overall, the description of Japanese policy toward the banks is centered on the Ministry of Finance, which has formal jurisdiction over Japanese participation in the banks, and the discussion of personnel policy is particularly useful to experts. The treatment of Japan's approach to the ADB and the EBRD is good; the discussion of Japan's conflict with the United States over World Bank lending policy is adequate, but it is not better than an account by Susumu Awanohara in a recent volume on U.S. and Japanese policy in multilateral institutions (see Peter Gourevitch, Takashi Inoguchi, and Courtney Purrington, eds., *United States–Japan Relations and International Institutions after the Cold War*, San Diego: Graduate School of International Relations and Pacific Studies, University of California, 1995).

The discussion of aid policy toward Russia yields useful information about Japanese bureaucratic politics and the limits of intervention by elected politicians, but the claim that this case yields special insight into Japan's multilateral diplomacy is not convincing. In this case Japan is pressured by the other Western powers to open up bilateral assistance to Russia; in response, Japan grudgingly cooperates with the West in aiding Russia through the development banks but refuses to extend large-scale bilateral aid due to an unresolved dispute over island territories. In essence, this is not so much a case of Japanese multilateral initiative as it is an effort to deflect Western pressure and criticism of its policy toward Russia.

The analytical chapters accompanying the empirical studies are less impressive. The book begins with a review of the literature on Japanese bilateral aid, and the discussion is hampered by an overly simplistic division of the literature into moralistic categories: Japanese aid as being either "evil" or "noble." This characterization fails to indicate the basic points being made in the literature and raises a question about the author's own understanding of how foreign policy analysis is to be done. The conclusion of the book is preoccupied with showing that Japan is becoming more activist in its foreign policy. The continuum between active and passive is divided into four categories, and there is a belabored discussion revolving around which category best characterizes Japan. The bottom line is that Japan took steps to raise its status in the pecking order in the World Bank and the ADB in the early 1990s but was not fundamentally interested in challenging U.S. leadership.

Societies and Military Power: India and Its Armies. By Stephen Peter Rosen. Ithaca, NY: Cornell University Press, 1996. 280p. $39.95.

Claude E. Welch, *State University of New York at Buffalo*

Rosen moves, in this intriguing book, away from the more "traditional" analyses of force and international security with which he has been associated, as exemplified in his influential *Winning the Next War: Innovation and the Modern Military* (1991). His concern in *Societies and Military Power* centers upon a classic chicken-and-egg issue: the interplay between underlying social structure and values and the shape of civil-military relations. He resolves this issue by wide-ranging inquiry into India.

On several occasions, Rosen warns us that his readings of Indian history are neither idiosyncratic nor deterministic. "History is full of contingent outcomes," he comments, "and nothing in this chapter or book is meant to suggest that social structures predetermine the emergence of a strong central state or its failure to emerge" (p. 141). *Societies and Military Power* sweeps over two and half millennia in one of the world's most complex, interesting societies. Has Rosen read too much into fragmentary documentation from the past? Has he skewed the evidence to reach a preordained outcome? I feared this all-too-common failing for a few score pages, then was persuaded by Rosen's interpretations.

Societies and Military Power asserts the following model, each element of which appeals to common sense but is difficult to demonstrate conclusively. (1) Social structures determine how individuals within societies treat one another; organizations enjoy some freedom to isolate their members from society. (2) Military organizations are *less* likely to reflect the structures of the larger society if they are relatively small and isolated physically, temporally, and/or psychologically. (3) The social structures of a political unit can affect its military power, offensive and defensive, by dividing the society as a whole and by creating divisions within the military. (4) The less a military reflects the structures of the society, the more it will be perceived as an alien element, unrepresentative of society and hence distrusted.

Central to recent models of civil-military relations has been what Luckham called "integral boundaries" (A. R. Luckham, "A Comparative Typology of Civil-Military Relations," *Government and Opposition*, Winter 1971, pp. 5–35). Modern Western societies characterized by "objective civilian control" (Samuel P. Huntington, *The Soldier and the State: The Theory and Politics of Civil-Military Relations*, 1957) are marked by "integral" boundaries between armed forces and the total social system. By contrast, settings such as India, historically and potentially contemporaneously, exemplify "fragmented" boundaries. Easy to assert and far more difficult to demonstrate, such models require careful empirical analysis. Detailed study of a single social system over time may provide the most satisfactory scientific foundation.

Rosen draws upon many years' interest and residence in India. Only a handful of specialists could rival the sweep of documentation used in *Societies and Military Power*. The historical sources are impressively numerous and varied in approach; but his choices are also highly—and I believe necessarily—selective. Following a summary of his model and an exegesis of the caste system, Rosen focuses upon four separate periods: ancient India (500 BCE–500 CE); Mughal rule (1526–1707, characterized as "the medieval period"); the initial 50 years of significant British overrule (around the late eighteenth century); and the five decades since independence. These chapters account for more than three-quarters of the text.

As may be expected, the closer the historical period to the present, the more copious is Rosen's documentation, the briefer the slice of time, and the more confident his conclusions (with the partial exception of the present, for the contemporary armed forces of India are shrouded in a thick veil of secrecy). For each period, Rosen uses apt, non-Indian comparisons. Relations between Indian militaries and society in the first period are briefly contrasted with Greek armies, notably that of Alexander the Great, and with (in more detail) the Roman republic before the first century CE. "Although the historical analysis tends to support the predictions generated by the general propositions," Rosen comments, "the record is not perfectly clear" (p. 103), with the important proviso that the strategic outlook of ancient India "was not uniquely Indian" (ibid.). The second period substantially overlapped the well-studied "military revolution" in Western Europe, the genesis of many elements of the contemporary state system, and the Thirty Years War. Technological differences between India and Europe at this time were not great, but contrasts in battle effectiveness were marked. Why? Rosen concludes this section more confidently: "These two factors—the differences in Indian and European societies and in the nature of their military organizations and their relationship to the host society—accounts [sic] for the difference in military performance" (p. 161).

For the third period, Rosen looks briefly to the Ottoman Empire and then at greater length to India, contrasting the effectiveness and power of the British-led East India Company forces (able to vanquish indigenous armies with ten times the personnel), on the one hand, with Mughal forces, on the other. "The British created an army in India that was significantly more powerful than the armies of the Mughal Empire because it was professional and as such not subject to the internal divisions always in operation in the Mughal army" (p. 196). Finally, in concentrating on the post-independence period, Rosen contrasts Indian and Pakistani civil-military relations and military effectiveness; he also provides an intriguing parallel with the Hapsburg Austrian empire, which "may provide the best comparison to test any conclusions about the consequences of separating an army from its social origins" (p. 254). The contemporary Indian Army remains "a collection of small, isolated, and inward-looking military communities" whose separation from society "preserved its strength and prevented its corruption" (p. 262), but this isolation is breaking down, at a cost to India's military effectiveness.

Not unexpectedly, Rosen finds his model explains many seeming anomalies in India's civil-military relations and security. Social structures are independent variables, "not simply a way to explain 'residual' variations in military power" (p. 258). Countries that fail to isolate their armed forces from social divisions reduce intramilitary cohesion and thus undercut military effectiveness. Yet, pronounced separation between society and armed forces results in mutual alienation, encouraging coups d'etat, protection of privilege, or marginalization (as Rosen aptly observes, no military officer may serve in the Indian Ministry of Defence, even after retirement).

Societies and Military Power should arouse discussion not only among India specialists but also among all students of civil-military and state-society relations. It is an important study. The initial skepticism with which I approached this book ebbed as Rosen displayed his historical evidence. He has written an unusual, challenging book, maintaining the high quality of the Cornell Studies in Security Affairs. *Societies and Military Power: India and Its Armies* provides ample food for thought and, I hope, an inspiration to further investigation of society-military interactions.

Win, Lose, or Draw: Domestic Politics and the Crucible of War. By Allan C. Stam III. Ann Arbor: University of Michigan Press, 1996. 239p. $44.50.

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Allan Stam argues that domestic politics and strategic decisions have a larger effect on the outcomes of interstate wars than realists or neorealists will typically acknowledge. Realist theoretical notions tend to emphasize the effect of relative military capabilities and alliances, while Stam argues that such factors as regime types, repression of internal dissent, and whether, for example, states decide to rely on military strategies emphasizing maneuver, attrition, or punishment will have a larger and more important influence on the outcome of wars. To evaluate his arguments, Stam analyzes data generated by the Correlates of War project regarding interstate wars occurring between 1816 to the late 1980s, as well as data such as those on strategies generated by the author. His analyses tend to show quite clearly that domestic political factors and choices of strategies are quite important in the determination of the outcome of wars.

There are some features of the book that are not as helpful to the reader as they might be. For example, the major hypotheses to be tested cannot be found in Chapter 2, "Hypotheses on the Outcomes of War." Instead, most of the hypotheses appear in the subsequent chapter, "Indicators of Costs and Benefits." Additional hypotheses appear in the chapter after that ("Testing the Model"), and the relationship between the two sets of hypotheses is not always clear. For example, in Chapter 3 one of the hypotheses is "the greater a state's population, the *less* the likelihood of victory" (p. 62, emphasis added). But in the next chapter it is hypothesized that "population levels should correlate with- . . . higher chances of victory (p. 98).

Near the end of Chapter 5, Stam asserts that "the potential problem of multicollinearity will be addressed in some detail later in this chapter" (p. 130). But this pledge is made at a point with only five more paragraphs remaining in the chapter. As best this reviewer can determine, multicollinearity is never mentioned again. (There is only one index entry.)

There may also be some lack of clarity about the cases, or the units and levels of analysis used in this book. Consider, for example, the following three hypotheses: "If a state achieves strategic surprise, it will be more likely to win" (p. 57); "the greater the imbalance of capabilities, the greater the chances of victory" (p. 61); and "wars fought on the appropriate terrain will be associated with victory" (p. 65). These hypotheses pertain to (1) individual states, (2) pairs of states, and (3) the entire set of states and/or coalitions involved in wars. Separately, these hypotheses are plausible, even intriguing. But combining all three in the same analyses can create questions about precisely what ideas are being evaluated.

At one point, for example, it is hypothesized that "the more democratic a state is, the more likely it will be to win" (p. 68). Later, it is stipulated that "higher degrees of democratization should be correlated with higher probabilities of victory" (p. 103). The prior hypothesis clearly suggests that democratic states are more likely than autocratic states to emerge victorious in wars. The second hypothesis can be interpreted to be identical in meaning. But the second hypothesis could also refer to the quite different idea that the more democratic a pair of states, or the set of states involved in a war are, the more likely that war is to produce a victor (regardless of the regime type of that victor), as opposed to a draw. The subtlety of the differences between such ideas and the fact that nearly identical language and terminology can be used to refer to both of them create ambiguities in the discussion on a fairly consistent basis. (There are also rather surprising omissions from the bibliographic references. Stam refers repeatedly, for example, to the propensity of democratic states to win wars without ever mentioning or citing David Lake's 1992 *APSR* article on the issue.)

One major point of the book is that a significant proportion of wars end in draws, yet theorizing about draws is rare. Stam analyzes 88 cases and concludes that 15 of them are draws (p. 119). As he acknowledges, these outcome codings differ rather substantially from the Correlates of War codings (which contain only two draws). Stam stipulates that "in a draw, both sides formally agree to cease fighting in an internationally recognized and binding treaty." He goes on to explain that "cases . . . where fighting ends . . . without internationally recognized or enforceable agreement to codify the outcome are coded as draws as well" (pp. 75–6). On the surface at least, what Stam appears to say here is that wars brought to a close with formal treaties, and those which are not, are counted as draws. Why these coding rules do not lead all wars to be counted as draws is not entirely clear.

The basic multivariate analyses in this book are based on 23 independent variables. This reviewer is not convinced that normal human beings or even professors at major research universities are truly capable of interpreting accurately or dealing simultaneously with all the implications of such complex models. There is admirable attention paid here to relative effects of the various variables, and Stam points out on occasion that his analyses suggest that choices of strategy tend to have a more important influence on outcomes than troop ratios or military ratios. What he actually reports, though, is that relationships between choices of strategy and outcomes are stronger than the relationships between capability ratios and outcomes when both types of variables are entered into the same analyses. Such results may indicate that advantages in capabilities lead to, or are at least associated with, choices of strategy, so that when strategy choices are controlled for, the relationship between capability ratios and outcomes is reduced or eliminated. Such results do not necessarily justify the conclusion that strategy choices are more important or have a larger effect. They may also be interpreted to mean that military or capability ratios are more fundamental to an understanding of war outcomes, with choices of strategy playing the role of intervening factors that are at least to some extent a reflection or a product of those capability ratios.

The methodological discussions in this book are enlightening. Readers are also provided with the opportunity to compare directly the different results produced by such techniques as multinomial logit analysis, ordered logistic regression, ordered probit regression analysis, and ordinary-least-squares analysis. The theoretical discussions are typically well informed and penetrating. If one is willing to assume that the complexities involved in the simultaneous analysis of 23 independent variables do not invalidate the results (a not unreasonable assumption), then the findings reported here may well constitute an important contribution to the field. In short, this book is a valuable addition to the literature, in spite of the fact that not all of its (inevitable) imperfections are of the trivial variety.

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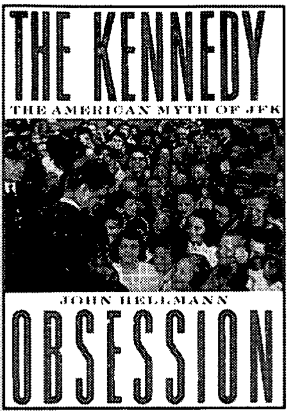
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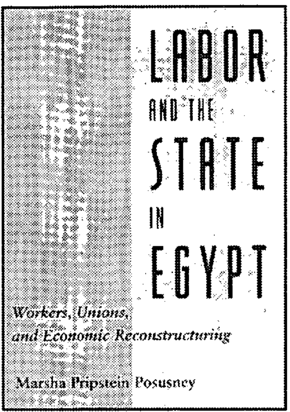
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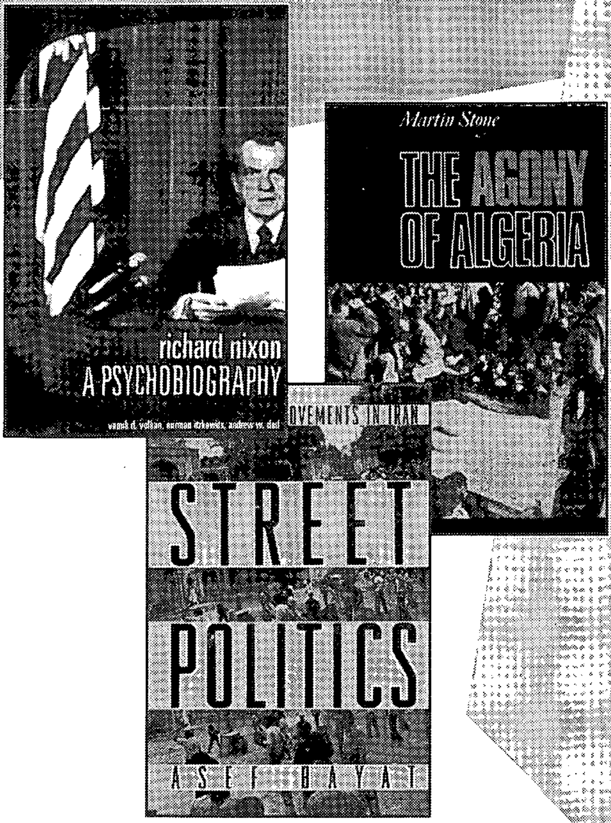
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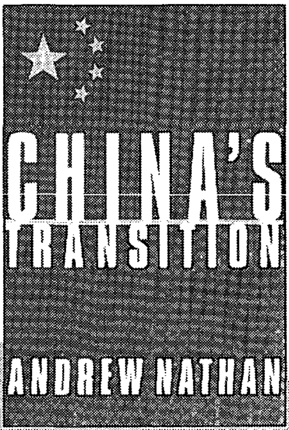

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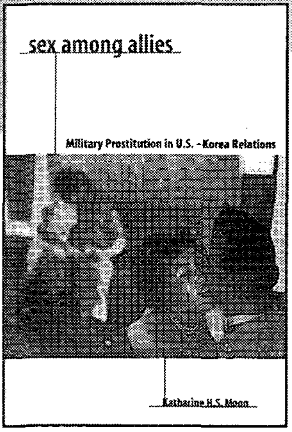

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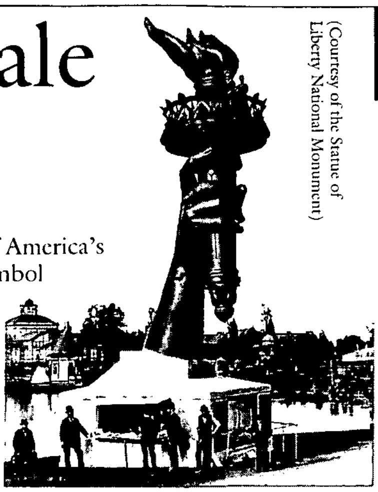

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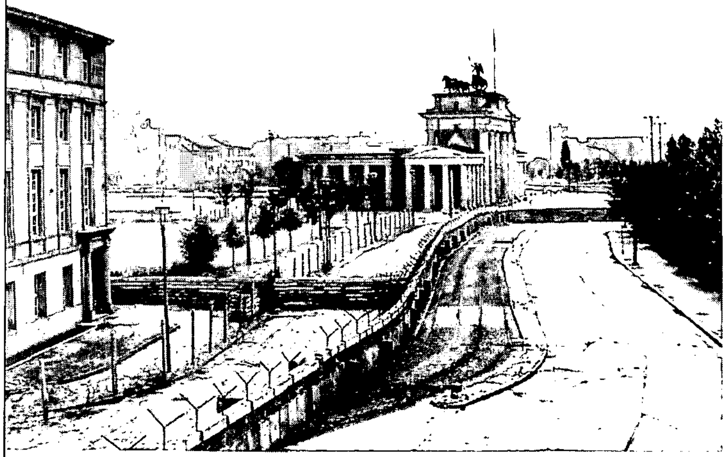

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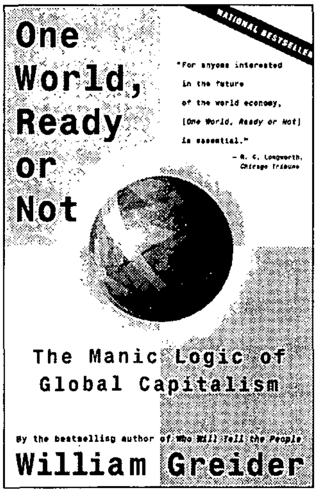
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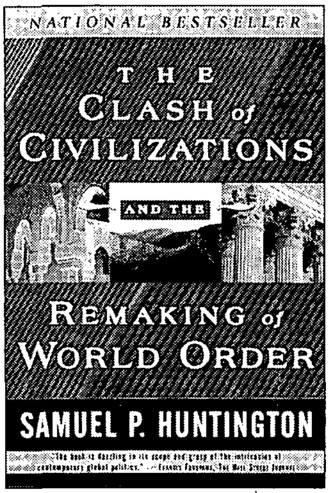
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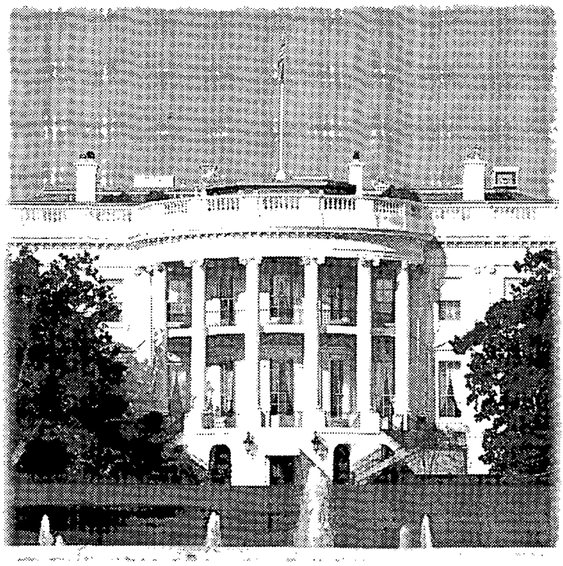

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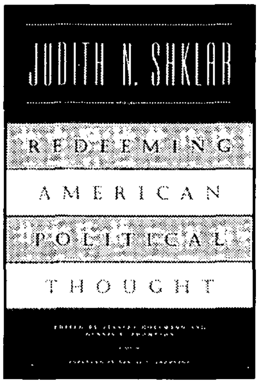

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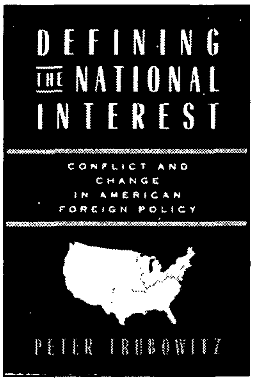

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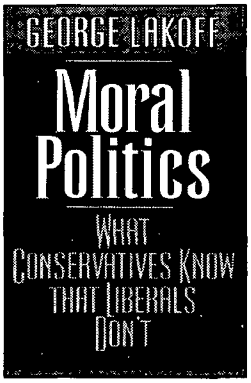

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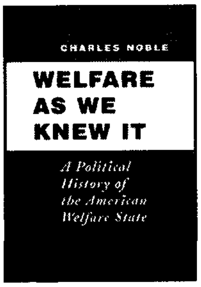

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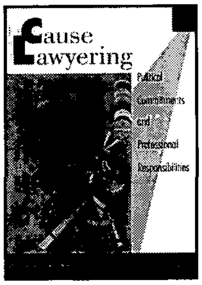

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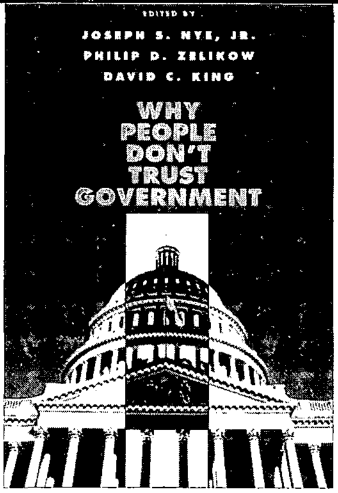

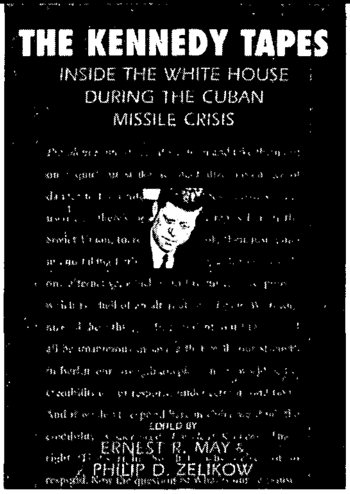

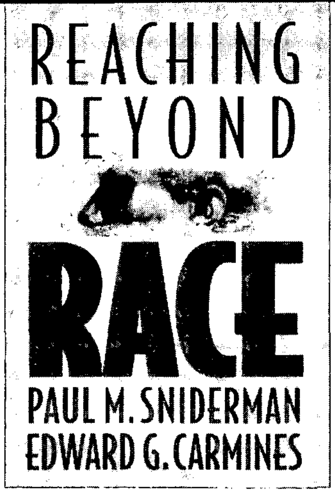

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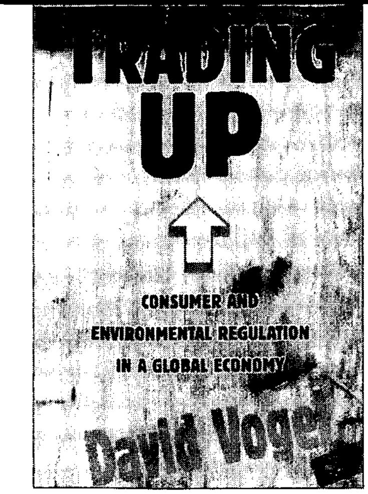

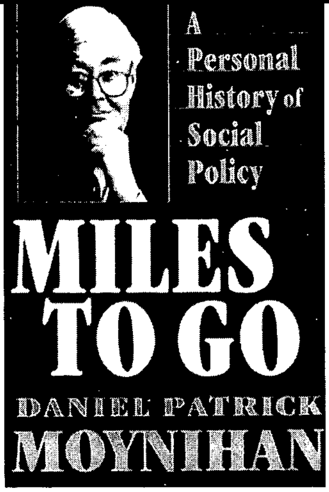

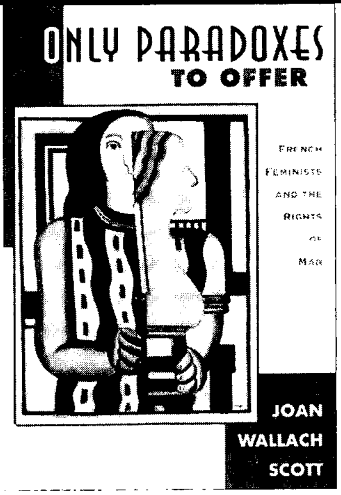

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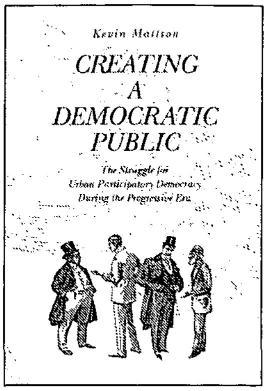
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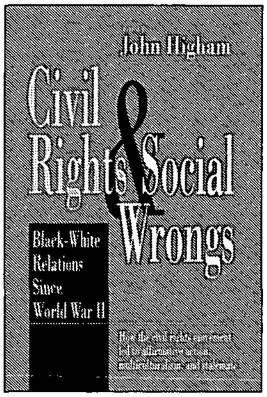
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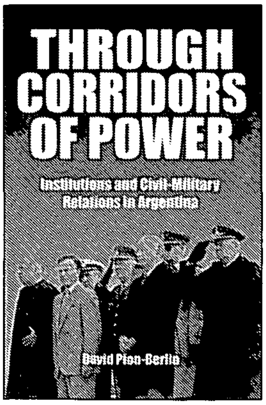
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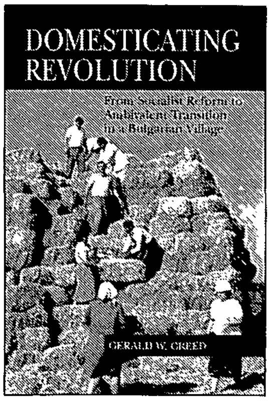
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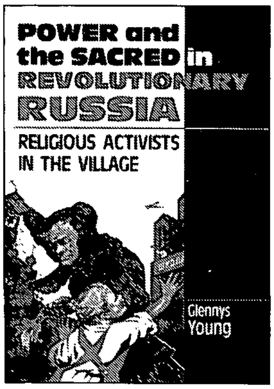
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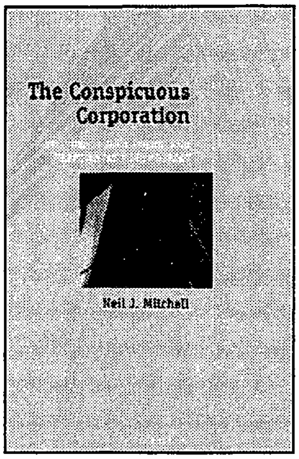
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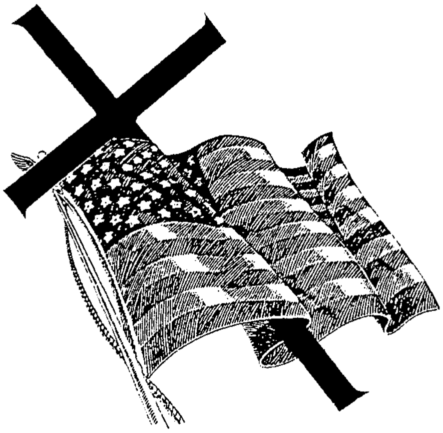

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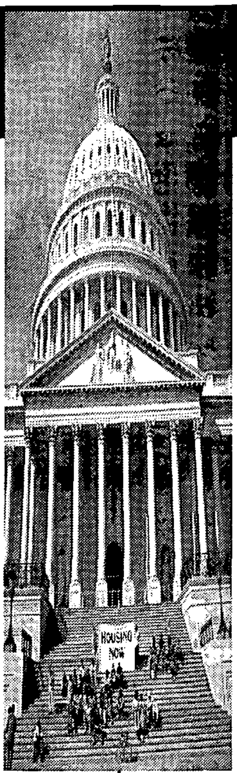

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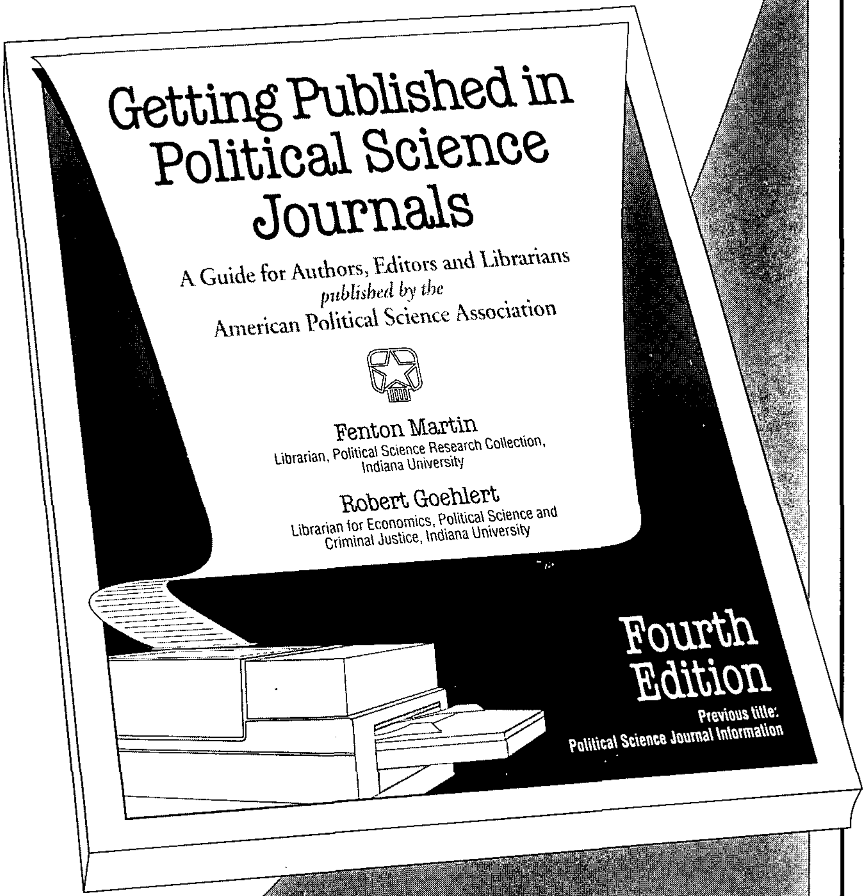

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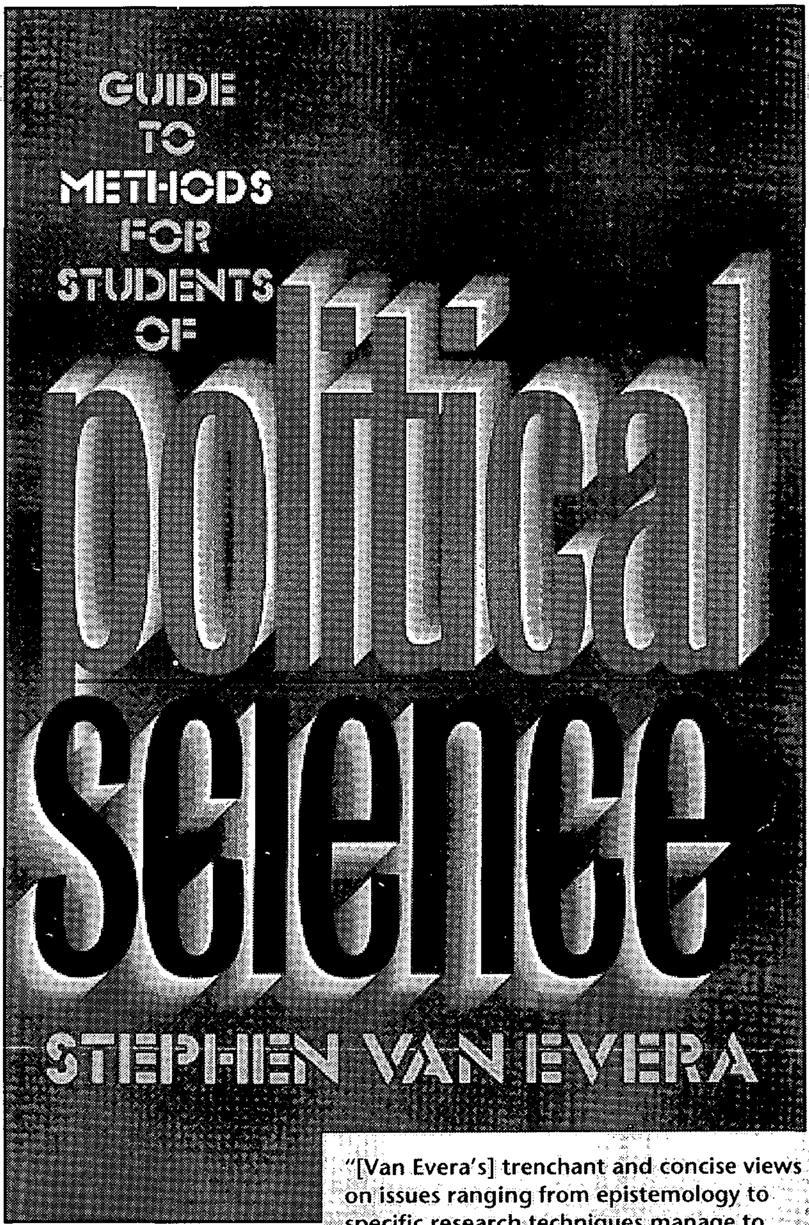

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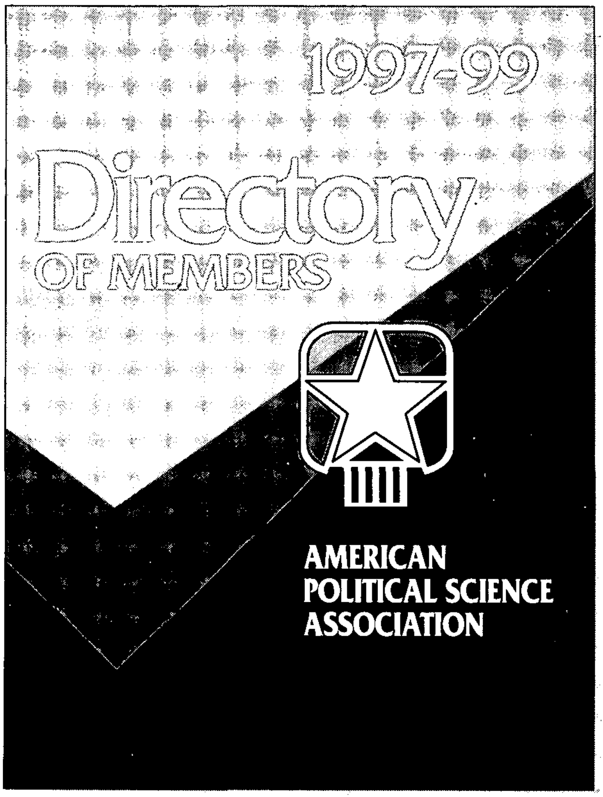

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